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ON CHONDRULES AND CHONDRITIC STRUCTURE IN METEORITES[1]

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The term "chondrit," from the Greek χονδρος, a grain, was first used, so far as I am aware, by Gustav Rose[2] to designate a class of stony-meteorites characterized by the occurrence of small granules or "*kugeln*." "Sie ist durch kleine Kugeln ausgezeichnet die aus einem noch nicht bestimmten Magnesia-Silicate bestehen, und in einem fein kornigen Gemenge eingemengt sind," etc. The word, with the addition of the terminal *e*, as *Chondrite*, has been very generally adopted, with its original meaning, by English and American writers. Unfortunately, as it would seem, a further modification of the word as *chondros*, *chondrule*, *chondrus*, or *chondrum* has been introduced, at first apparently synonymous in meaning with *kugel* as used by Rose though it is to be noted that he did not define the word quite as clearly as might be desired. He wrote: "* * * in Bruche erscheinen sie *theils uneben*, *theils fasrig*, im letzern Fall jedoch stets nur sehr feinfaserig, indessen doch immer bestimmt erkennbar fasrig, besonders unter der Lupe * * nie radial, sondern immer excentrisch fasrig." No further reference is made to those of "*uneben* Bruche" and one is left only to surmise that they may have been of a granular or porphyritic rather than fibrous structure. The fact that Rose's work was written before the day of thin sections doubtless accounts for the undetermined character of the magnesian silicate.

Tschermak in his *Mikroskopische Beschaffenheit* (1885) was little more explicit in his use of terms than was Rose. He wrote: "Kugelchen und *überhaupt rundliche Körper*, welche bald aus einem einzigen krystallindividuum, bald aus mehreren bestehen, öfters auch aus verschiedenen Gemengtheilen zusammengestzt sind, bilden das Gestein fast allein (Borkut) oder sie lagern unverletzt, öfters auch zersplittert in einer lockeren bis festen Tuffmasse." Elsewhere he includes all the rounded forms under

the term *chondren*, though in his plate legends and descriptions he designates both as *Kugelchen*, thus using the two terms synonymously.

A perusal of the literature shows that by English and American writers, the terms *chondrule*, *chondrus*, *chondrum* or *chondros* are now and have for some years been applied to the rounded and oval granules presenting a considerable range in mineral composition and still wider range in internal structure, thus making the terms synonymous with kugel or kugelchen as used by Tschermak above. Of later years and as illustrated in the generally adopted scheme of classification,[3] there has seemed a disposition to use the term kugel in a descriptive adjective sense, as *kugelchen chondrit*, under which name are included stones containing chondrules (or chondri) having a radiate structure—the *spherulitic*[4] chondrites of American writers. There has thus apparently arisen in the minds of many a confusion which, as it seems to the writer, has been in part at least responsible for the diverse views expressed concerning the origin of these peculiar bodies. In other words, there has been a failure to recognize or discriminate between the *kugelchen* with radiate structure and the often irregular polysomatic forms with the "uneben Bruche."

The present paper, then, represents an attempt on the part of the author to make this discrimination and to show how far proposed theories may apply to the various forms presented.

NATURE OF THE CHONDRULE

At the outset and for the purpose of making clear what is to follow, it will be well to figure and describe a few characteristic forms of the individual chondrules. This notwithstanding the previous most excellent and comprehensive work of Tschermak and Cohen.[5]

Mineralogically, the chondrules, using the word in its broadest and most comprehensive sense, in nearly all meteorites are composed chiefly of the minerals olivine or pyroxene, the latter in either orthorhombic or monoclinic forms, or both. Some are largely of an undifferentiated glass. Feldspars occur but rarely except in the form known as maskelynite. In addition are occasional enclosures of metal or metallic sulphides, chromite or other minor constituents. The metallic iron sometimes occurs in rounded chondrite-like blebs, though it is doubtful if this should be referred to under that name. Structurally, the chondrules in the same meteorite may vary from densely cryptocrystalline, almost amorphous, to those that are part glassy and porphyritic or even holocrystalline.

I. *Glassy, Cryptocrystalline and Radiated Forms.*—In figures 1 and 2, are shown examples of cryptocrystalline forms from the stones of Barratta, Australia, and Cullison, Kansas. That of figure 1 is of a peculiar brownish translucency and very dense, resembling the "felsitic" structure of the early petrologist. In the Cullison stone, figure 2, the chondrules, also of a brownish color, are not completely isotropic but between crossed

nicols break up into several illy defined areas over which the dark cloud sweeps faintly and irregularly as the stage revolves. The material seems to be a partially devitrified glass in a condition of optical stress as from

FIG. 1

sudden cooling. Chondrules of this type and those next to be described more nearly resemble the spherulites of the terrestrial rocks than any others which have come under my notice. Their outlines are at times as sharply demarked from the matrix in which they are embedded as are

FIG. 2

the spherulites in the rhyolitic obsidians of the Yellowstone National Park.

Chondrules of the radiating type are shown in figures 3, 4 and 5, from the

meteorites of Elm Creek, Hessle, and Parnallee. The mineral in all cases is enstatite[6] and the outline of the spherule as sharp and clean as though it had been turned on a lathe. In the Elm Creek example crystallization

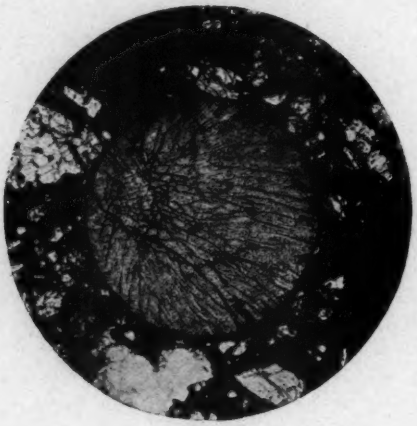

FIG. 3

evidently began at one point on the surface of the spherule and extended inward throughout, but the cooling proceeded too rapidly for the production of an optically perfect crystal. In the Hessle stone (fig. 4), there were evidently several initial points of crystallization. Forms like these

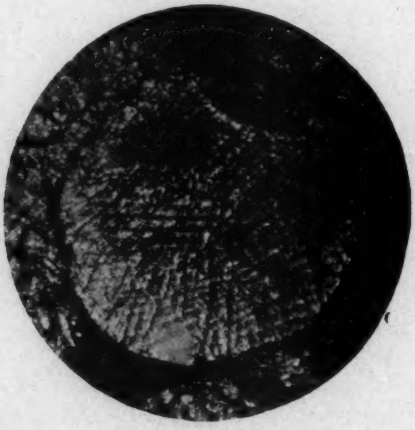

FIG. 4

grade imperceptibly into such as are shown in figure 5, in which the radiating bars have unmistakably the crystallographic properties of enstatite.

II. *Half Glassy, Barred and Porphyritic Forms.*—Porphyritic forms are

characteristic of both olivine and enstatite chondrules, while the barred forms, such as are shown in figures 6 and 7, are limited mainly, if not wholly, to monosomatic forms composed of olivine. In figure 6, from the

FIG. 5

Beaver Creek stone, the white portions are olivine which extinguish practically as a single unit; the black portions are glass. It is to be noted that the outlines, though sharp are not smooth as in those described above, but have projecting particles extending out into the ground; also

FIG. 6

that this border portion often contains enclosures. In figure 7 from the Hendersonville stone, the bars are bent and curved and do not all extinguish simultaneously, as the stage is revolved, the dark cloud sweeping

over it irregularly, indicating a condition of stress. Here, as in the last, the border is not sharply demarked from the ground and it is often impossible to state if a certain crystal particle belongs to one portion or the

FIG. 7

other. It should be noted that this stone is a crystalline spherulitic chondrite. According to Tschermak, in chondrules of this nature the olivine bars are sometimes interlaminated with plagioclase (e.g., in the Dhurmsala stone). In the porphyritic form shown in figure 8 from the

FIG. 8

Tennasilm stone, the granular ground abuts sharply against the black glass of the chondrule with only on one side a manifested tendency to penetrate into and beyond the border. It is to be noted that the enstatite

phenocrysts within the chondrule and near the border are often cut off sharply as though the sphere, originally much larger, had been uniformly reduced by abrasion. This will be referred to later.

FIG. 9

III. *Holocrystalline Chondrules.*—As would naturally be expected, these porphyritic forms, through a reduction of the proportional amount of glass, pass gradually into those which are almost or quite holocrystalline and polysomatic as shown in figures 9 and 10 from the Barratta and Elm Creek

FIG. 10

stones respectively. Of peculiar interest are those of the polysynthetically twinned pyroxene (fig. 10). For some unexplained reason, these rarely grade into the half glassy porphyritic forms, the entire chondrule consist-

ing of the closely crowded pyroxenes with comparatively little, if any, interstitial glass. In figure 11, from the Parnallee stone, it will be noted the crystals are in some instances slightly curved, their vertical axes lying approximately parallel with the circumference of the circle

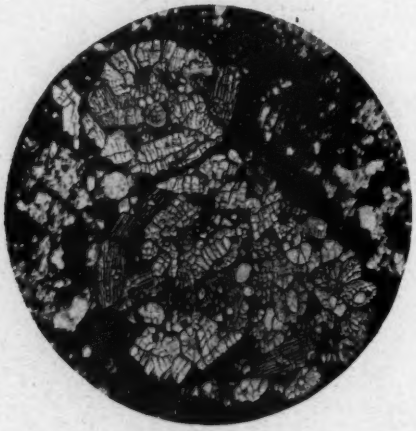

FIG. 11

which forms the border of the section. The appearance is as if the chondrules had been molded by external forces after the crystals had formed but while yet in a more or less plastic condition. Again, the pyroxene crystals abut sharply against the border and are cut off at the margin as

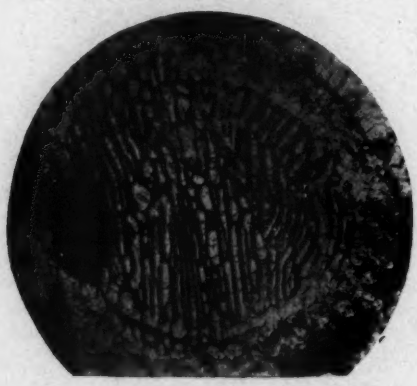

FIG. 12

in the half glassy, porphyritic forms mentioned above, and as shown in figures 10 and 11. Occasional forms are met with which have all the appearance of fragments, slightly rounded, of holocrystalline granular rocks, which as noted later, I believe them to be.

IV. *Secondary Borders about Chondrules.*—A not uncommon feature of the chondrule is the narrow border or rind about the circumference. These borders as a rule, are of lighter color than the interior, of a clear, more pellucid nature, though it may be including portions of the minerals characteristic of the matrix in which they are embedded. This is well shown in the olivine chondrule, figure 12. This border has an appearance at once suggestive of the secondary intergrowth or enlargement often seen in feldspars and other minerals of terrestrial rocks. The later portions sometimes, though not always, have the same optical orientation as the interior. I am not certain if this border is of a like nature to that described by Tschermak about some of the chondrules of the Grosnaja stone and which he considered of secondary origin. In some instances

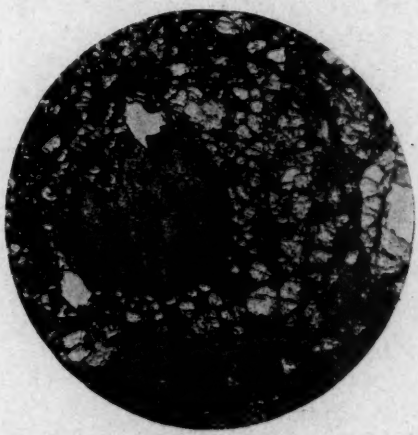

FIG. 13

the chondrules are surrounded by an irregular border of metal or metallic sulphide.

V. *Double or Compound Chondrules.*—Occasional forms are met with in which a large crystal of olivine or pyroxene is inclosed by a border of finer crystals of the same mineral but suggestive of a later generation. More common are forms such as are shown in figure 13. The crystals are of enstatite but the larger one is filled with minute cavities which are lacking in the smaller forms. Of greater interest is the occasional occurrence of a chondrule within the mass of a second or larger form, as figured by Tschermak, on plate 8, figure 1, of his *Beschaffenheit.* I will refer to this also, later.

The descriptions thus far given are of the most perfectly formed and outlined chondrules only. The broken, angular and, in some cases, distorted radiate enstatite forms are plainly mechanical derivatives, and while they have a bearing upon the origin of the masses in which they occur,

they can, except as noted below, have little bearing upon the subject of the origin of the chondrules themselves. They are, therefore, passed over for the present.

THE ORIGIN OF THE CHONDRULE

I. *Theories of Origin.*—In this review it will perhaps not be necessary to go back much beyond the time of the introduction of the microscope and thin sections into the study of rock structures since obviously little that was accurate could be told of them by the naked eye alone.

A brief glance at the literature is sufficient to suggest that many of the opinions expressed have been based upon examinations of but a limited number of occurrences which quite failed to yield the information necessary for building satisfactory hypotheses or conclusions.

Reichenbach, as early as 1860[7] wrote, "Aus allerdem wird es klar, das die Einschlusse in den Meteoriten, als die Trümmer und die geschicbartigen Knollen und Kugeln darin, keine einfach nachen Bestantheile, sondern nichte anderes sind als auch wie die Meteoriten. Meteoriten nur von anderer Anordung ein und derselben naturn Bestandtheile" And again, "Es sind also die Einschlusse theils kleine meteoriten, theils Trümmer von meteoriten von hohenen Alter als diejenigen meteoriten es sind, in welche sie eingeschlossen vorkommen; es sind altere kleinere meteoriten in jungerern grossern meteoriten." In brief, and in plain English, he believed each particle as now found to represent a minute but independent meteorite derived from the breaking up of some older preëxisting stone and now included as a constituent part of one new formed.

In discussing the microscopic structure of meteoric stones, H. C. Sorby, in 1864, wrote,[8] "It would, therefore, appear that, after the material of the meteorites was melted, a considerable portion was broken up into small fragments subsequently collected together, and more or less consolidated by mechanical and chemical action. * * * Apparently this breaking up occurred in some cases when the melted matter had become crystalline, but in others the form of the particles lead me to conclude that it was broken up into detached globules while still melted. This seems to have been the origin of some of the round grains met with in meteorites; for they occasionally still contain a considerable amount of glass, and the crystals which have been found in it are arranged in groups radiating from one or more points on the external surface in such a manner as to indicate that they were developed after the fragments had acquired their present spheroidal shape." In continuation of this same idea in 1877,[9] Sorby wrote: "As is well known, glassy particles are sometimes given off from terrestrial volcanoes, but on entering the atmosphere they are immediately solidified and remain as mere fibres, like *Pele's hair*, or as more or less irregular laminae, like pumice dust. The nearest approach to the globules in meteorites is met with in some artificial products. By directing a

strong blast of hot air or steam into melted glassy furnace slag, it is blown into spray, and usually gives rise to pear-shaped globules, each having a long, hair-like tail, which is formed because the surrounding air is too cold to retain the slag in a state of perfect fluidity. Very often the fibres are of the chief product. I have never observed any such fibres in meteorites. The formation of such alone could not apparently occur unless the spray were blown into an atmosphere heated up to near the point of fusion, so that the glass might remain fluid until collected into globules. The retention of a true vitreous condition in such fused stony material would depend on both the chemical composition and the rate of cooling, and its permanent retention would in any case be impossible if the original glassy globule were afterwards kept for a long time at a temperature somewhat under that of fusion. The combination of all these conditions may very well be looked upon as unusual, and we may thus explain why grains containing the glass are comparatively very rare; but though rare they point out what was the origin of many others. In by far the greater number of cases the general basis has been completely divitrified, and the larger crystals are surrounded by a fine-grained stony mass. Other grains occur with a fan-shaped arrangement of crystalline needles, which an uncautious, non-microscopical observer might confound with simple concretions. They have, however, a structure entirely different from any concretions met with in terrestrial rocks, as for example that of oolitic grains. In them we often see a well-marked nucleus, on which radiating crystals have been deposited equally on all sides, and the external form is manifestly due to the growth of these crystals. On the contrary the grains in meteorites now under consideration have an external form *independent of the crystals* which do not radiate from the centre, but from one or more places on the surface. They have, indeed, a structure absolutely identical with that of some artificial blowpipe beads which become crystalline on cooling. With a little care these can be made to crystallize from one point, and then the crystals shoot out from that point in a fan-shaped bundle, until the whole bead is altered. In this case we clearly see that the form of the bead was due to fusion, and existed prior to the formation of the crystals. The general structure of both of these and the previously described spherical grains also show that their rounded shape was not due to mechanical wearing. Moreover, melted globules with well-defined outline could not be formed in a mass of rock pressing against them on all sides, and I, therefore, argue that some at least of the constituent particles of meteorites were originally detached glassy globules, like drops of fiery rain." In this Sorby would appear to have had reference only to *kugels* with radiate, internal structure.

Tschermak, who together with Haidinger, was one of the first to pronounce on the tuff-like character of the chondritic meteorites, announced in 1874[10] the opinion that the individual chondrules (kugelchen) were but

rock particles which became simply rounded under conditions similar to such as might exist in the throat of a terrestrial volcano. "Ich widerhole hier nur das Eine, dass Ich die Chondrite fur Zerreibungs-Tufe, und die Kugelchen derselben fur solche Gesteinspartikelchen hatte, welche wegen ihrer Zähigkeit bei Zerreiben des Gesteines nicht in Splitter Aufgelöst, sondern, abgerundet wurden." And again in 1875[11], "Man kann sich allenfalls vorstellen das die Steinmassen, welche der Zerreibung ausgesitz weren, ziemlich weich gewesen sein und würde sich dadurch der Vorstellung Daubrees nähern, welcher an ein Gestein denkt welches in einer Gas masse wirbelnd erstarrte; doch is es sicher, das die Kugelchen das Resultat einer Zerreibung sind." In both of these quoted expressions Tschermak seems to have had in mind only the granular and porphyritic polysomatic forms, and the fragmental "kugelchen."

Three years later[12] after a consideration of the depressions and excrescences occurring in and on the chondrules of the Tieschitz meteorite, he came to a partial agreement with Sorby conceiving that "Die Kügelchen sind nach wie vor wegen der tuffartigen Beschaffenheit der Meteorsteine als Resultate volcanischer Eruptionen und Explosionen anzusehen, aber ihre Form dürfte doch eher von einem plastischen Zustande, als von der Zerreibung starrer Partikel abzuleiten sein." And again, after another four years[13] he announced, "Ich hatte * * * zu der Ansicht gefuhrt wurde, dass die Kügelchen der chondrite als erstarrte Tropfen anzusehen sind, während die aus Splittern bestehende Grundmasse nach wie vor als vulkanischer Detritus zu betrachten wären."

Daubree[14] seems also to have held the opinion that the majority of chondrules were simply debris particles rounded by attrition. "J'ai montre," he wrote, "que la structure globulare telle qu'elle se presente dans certains types * * * a ete imitee artificiallement, et s'explique par une sort de granulation, aperee au moment ou la substance se solidifie. Mais le plu souvent, les globules des meteorites parraissent etre des simples debris arrondis par frattiment."

F. Rinne[15] by means of a simple electric device was able to fuse the silicate constituents of meteorites and by abrupt alterations of the strength of the current produce a "spratzen" of the melt resulting in the projection from the crucible of small drops which quickly cooled in the form of "kugels." To some such action he would ascribe the formation of meteoric chondrules. Later, by the aid of an oxygen blast and a Linnemann burner he was able to produce enstatite beads evidently in every way comparable with meteoric chondrules.[16] These it will be observed are really synthetic demonstrations of the possible correctness of Sorby's views. Berwerth in 1901[17] announced his conviction that the chondritic stones were tuffs more or less completely metamorphosed by heat, and seemed to regard the individual chondrules as portions of the melt that cooled in globular form. Borgström[18] in his description of the Hvittis

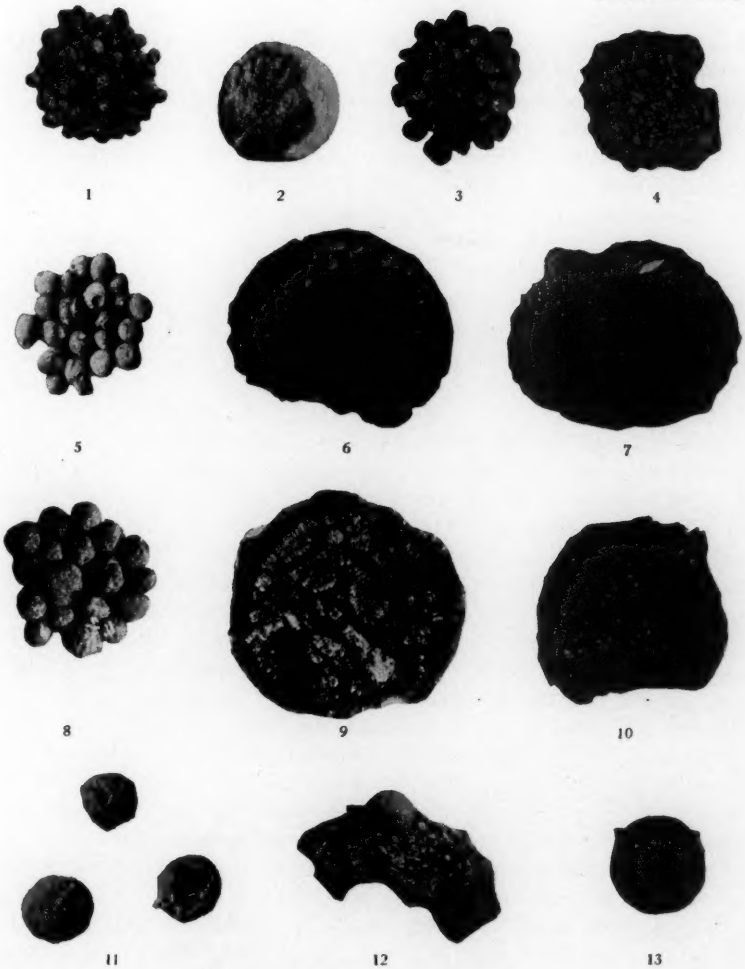

PLATE 1

Fig. 1.—Cluster of smooth chondrules out of Allegan stone.
Fig. 2.—One of the same chondrules broken and showing radial structure.
Fig. 3.—Cluster of rough chondrules out of Allegan stone.
Fig. 4.—Thin section of one of same.
Fig. 5.—Cluster of smooth chondrules out of Bjurböle stone.
Figs. 6 and 7.—Thin sections of same.
Fig. 8.—Cluster of rough chondrules out of Bjurböle stone.
Figs. 9 and 10.—Thin sections of same.
Figs. 11–13.—Chondrule-like bodies from Bessemer convertor, Sparrow Point, Md., Steel Works.

stone (1903) (a crystalline chondrite), states that the chondrules are always so firmly intergrown with the ground that it is often impossible to determine where the one leaves off and the other begins. In many instances, the enstatites of a chondrule extend out into the ground mass with which they are intergrown. As noted, the Hvittis stone is a crystalline chondrite; this might suggest either a crystallization of the chondrule *in situ* or a case of secondary enlargement. In writing of the Shelburn stone, a grey chondrite, however, he says,[19] "Each individual chondrule represents a structure of cooling and crystallization from a molten state, and as their structure shows an intimate relation to the boundary of the chondrule it must be supposed that each, at the time of its solidification, was a separate unit. Because chondrules of the same chemical composition have a different structure, they must have been formed under different

FIG. 14

physical conditions. Since such a variety of conditions cannot have existed in the narrow space in which the different structures now are met with, the chondrules must have accumulated after solidification." Such a condition is well shown in figure 14, from the stone of Cedar, Texas. Meunier[20] basing an opinion apparently upon the theoretic work of M. Faye suggests the probability of the chondrules resulting from the sudden condensation of a cyclonic vapor. "Il parait difficile de ne pas admettre que les chondres sont aux roches de precipitation gazeuse ce que les dragees de Carlsbad et le fer en grains sont aux roches de precipitant aqueus * * * Conformement a la terminologie dont font usage les paleontologistes a propos du vent fossiles du soleil fossile, de la pluie fossile, on serait tente de les qualifier de *cyclones photospherique fossiles*." This is conceivable, to the present writer, only in the case of radiate enstatite or monosomatic forms.

Brezina, to whom is so largely due the building up of the magnificient collection at Vienna, concludes a review[21] of the subject with the statement, "Durch die vorangefuhrten Beobachtungen können wohl die alteren Ausschauugsweisen als beseigtig betrachteh werden, und wir können wohl mit Bestimmtheit die Meteoriten als gestörge über hastete Krystallbildungen in einem einzigen gemengten magma bezeichen." This I understand includes both the ground mass and its chondrules.

Hussak, basing an opinion on experimental work by himself and Dolter[22] suggests that chondritic meteorites, like that of Uberaba, Brazil (a crystalline chondrite), originate through the long continued immersion of meteoric stones in a nickel-iron-rich magma, and are to be regarded as true volcanic ejectamenta, "Ich möchte demnach die Meteorsteine durch ultrabasische Eruptivgesteine vergleichen und die Bildung der Chondren wie der Trümmerstrutur und der schwarzen Adern als eine magmatische Eirnwirkung vor der Ejektion ansehen. Daher die vollstandigen Uebergange in Siderite und die Deutlichen Korrosionserscheinungen an den grossen Olivinkristallen der Pallasite."

C. Klein, in 1906,[23] evidently basing an opinion largely on figures of chondrules in the works of Hahn and Tschermak, affirmed that there occur many ideally perfect forms that lack the eccentric radiating structure, but are "radial strahlig," from a centre, equally in all directions and are *true spherulites*. Those not having this perfection of structure are considered fragments. It may be well to note before going further, that Klein apparently stands alone in holding these views though they may be correct for certain forms.

Wahl[24] would explain the formation of the chondrule as due to the cooling of a silicate melt in a heated atmosphere, the resultant drop crystallizing from the surface inward. "Die Entstehung der Chondren lässt sich also ganz allemein als durch Zerstäubung von Silikatschmelz fluss innerhalb einer heissen Atmosphäre und Kristallization der hierdurch enstandenen Tropfen von aussen nach innen zu erklaren." This again would seem to refer only to the cryptocrystalline, radiating enstatite, and the barred and monosomatic olivine chondrules.

Finally, in 1913, Fermor,[25] of the Indian Survey, suggested that the chondrules are remelted garnets.

In my preliminary considerations of this subject, I felt that in order to arrive at a solution of the problem it would be necessary to begin with a study of the crystalline chondrites, since in case the chondrules were spherulites, it would be here that they would be found in their primary condition of development. A study of all the thin sections of stones belonging to this group, now in the Museum collections has, however, brought about a change of view since in none of them do I find chondrules developed in the variety and perfection of forms existing in those meteorites which are plainly tuffaceous. This fact and others to be mentioned later have

led me to regard the larger part if not all chondritic stones as originally tuffaceous and owing their more or less crystalline condition, where such exists, to heat and pressure in a nonoxidizing or even reducing atmosphere. The study of the origin of the chondritic meteorites must begin then with the study of the chondrules themselves as found in the tuffs and a gradual tracing of them back through the crystalline "kugelchen" forms to the holocrystalline types.

Disregarding the fragmental, angular and more or less splintered radiate forms concerning the fracturing and secondary origin of which there can be no doubt, and confining ourselves to those which so far as can be determined have the form and structure assumed at the time of their formation and are of a more nearly perfect spherical, oval or "kugelchen" shape, their study in thin section brings out very clearly two important fundamental distinctions. 1st. The most perfectly spherical and oval forms, sharply differentiated from the matrix, are those showing a cryptocrystalline or radiating internal structure, and are mineralogically of pyroxene (enstatite or bronzite). These forms often show excrescences or saucer-shaped depressions, as through shrinkage or interference during solidification. 2nd. Those of a polysomatic nature, composed of phenocrysts of olivine or pyroxene in a more or less glassy base, or of an almost holocrystalline aggregate of one or more minerals, are of irregular outline, have more the appearance of fragments and never show the saucer-shaped depressions and excrescences. Further than this, it becomes very quickly evident that a theory of origin which will account for the first, must fail in the case of the second. It is not possible for instance, that a chondrule of phenocrysts in a glass base like that shown in figure 8, should have originated under the same conditions and in the same manner as that in figure 3.

It is possible to conceive of a basic magma containing the necessary constituents for the formation of olivine or enstatite to be in a sufficiently liquid condition to cause, or allow it, when thrown into a hot or thin atmosphere, to take the form of spherical drops which on cooling will become glassy or crystalline, according to conditions. Should the drop begin cooling on one point of its surface, the crystals might radiate from this point outward, or rather inward with reference to the chondrule, giving rise to the radiated form like those of the Elm Creek and Parnallee stones (figs. 3 and 5). In case the cooling began at several points on the surface, the interior of the chondrule would be broken into areas each of which represents an independent crystallization, as shown in figure 15. It would thus be fairly easy to account for the monosomatic glassy, cryptocrystalline and radiate forms[26] (Kugels), and even the barred forms so characteristic of olivine chondrules in the stones of Hendersonville and Tennasilm.

The porphyritic polysomatic forms composed of phenocrysts of pyroxene

or olivine in a glass base like those of Tennasilm (fig. 8), or Cabarrus (Monroe) offer, as it seems to me, insuperable difficulties to any such theory of origin and I am unable to agree with Dr. Wahl in his, at first

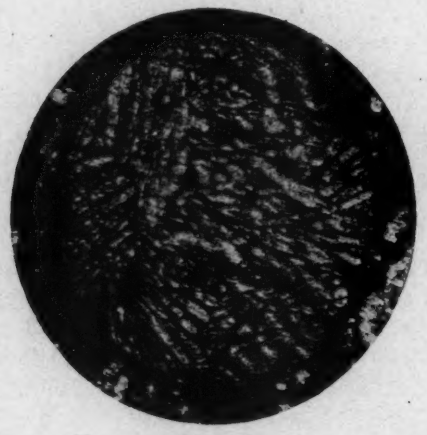

FIG. 15

thought, lucid and satisfactory explanation. It is obviously impossible that such could have originated from the cooling from the surface inward of a molten drop. Even in the hot air of Dr. Wahl's imagination the crys-

FIG. 16

tallization must almost certainly have begun at the surface and progressed inward giving rise to an exterior crust or border as in figure 16. This seems almost too self-evident to need mention. One must consider such

(i. e., the porphyritic forms) in the light of knowledge gained from a study of terrestrial rocks, and as due to the gradual cooling of a melt of some considerable proportions to the point of crystallization of the phenocrysts, followed by an abrupt refrigeration resulting in the production of the glassy base. This crystallization and final cooling can scarcely have taken place after the drop or chondrule took on its present form. It would seem doubtful, also, if a melt which had cooled sufficiently to allow the formation of the phenocrysts would be sufficiently fluid to permit the formation of the spherical drops under any probable conditions. It is true the separating out of the crystals would leave a magma more fusible than was the original, but it would still require an almost impossible condition to bring about a formation of the pellets as suggested.

It is questionable further if conditions could have been such as to bring about a practically instantaneous, or rather simultaneous cooling throughout the entire chondrule. If cooling as an isolated drop of molten matter in which were floating the phenocrysts there would seemingly, almost for a certainty, be occasional indications of an earlier cooling on the outer surface manifested by the formation of the crust as already mentioned, or conditions of strain or, possibly, signs of incipient crystallization. Such I do not find, the glass being uniform in physical properties and apparently in composition from centre to circumference of the chondrule as it appears in the section. The same holds true of chondrules which are nearly or quite holocrystalline, like those of twinned pyroxene figured[27] by Tschermak from the Renazzo stone, or those from the Cullison, Kansas, stone described by myself.[28] In these cases one has to consider a much more uniform and gradual reduction of temperatures, such as would give the crystallographic forces full opportunity to perform their task. But were the material in the form of an isolated drop, and the forces given sufficient time the exterior form of the aggregate can scarcely be conceived as smoothly spherical, but must have been irregular with numerous projecting angles such as are frequently to be seen in spherical concretions of pyrite and marcasite. Such forms I do not find though I have sought for them diligently. In forms like figures 8, 9 and 10, it is, I think, self-evident that the original surface has been reduced by attrition, and whatever character it may have had has been lost. This same probability of reduction in size is suggested by the twinned pyroxene chondrules in the Parnallee stone. It is obvious that such cannot have resulted from the cooling and crystallization of a molten drop unless it were one of considerable greater magnitude. The occasional curved-enwrapping character of the outer crystals (fig. 11) can only be accounted for on the supposition of Tschermak and Daubree, already noted, that the material was somewhat soft and plastic.

It is notable further, so far as my own observation goes and so far as

I can learn from the descriptions given by other writers, that the peculiar saucer-shaped depressions (fig. 4, p. 452 and fig. 5, pl. 1) which were surely formed at the time of solidification are limited to the cryptocrystalline and radiated enstatite types. They are never found in the polysomatic, porphyritic and holocrystalline forms although these may be of like mineral composition. The cause of this cannot then be chemical. It would seem fair to assume that it was fundamental and due to their origin under quite different conditions.

FIG. 17

Chondrule-like blebs of metallic iron in furnace slag showing, in some instances, depressions similar to those of the enstatite chondrules in meteorites. The white pointers mark some of the more prominent forms.

Concerning the origin of these depressions and indeed as bearing upon the general subject of chondritic formation, it may be well to add incidentally that in a fragment of furnace slag received from Lawrence County, Tennessee (exact locality unknown), I find abundant beautifully spherulitic chondrules of metallic iron embedded in the glassy slag. These are from one to three mm. in diameter and often so densely crowded as to have mutually interferred so that when separated there is left on one of them a concavity similar to that of the enstatite chondrules. Indeed the simulation of exterior form is almost perfect (see fig. 17). If, however,

one were to assume that the mode of formation of the concavity was the same in the two cases it will apparently be necessary to further assume that the chondritic forms, the fused drops of "fiery rain," accumulated in such quantities before all were fully indurated as by their weight to allow those most firm to indent those still plastic. Were it not for the rarity of the double or excrescent bearing forms referred to elsewhere, it might be suggested that the concavities were due to the breaking apart, one from another, after solidification. It should be added that I have not yet found any evidence even suggesting that the enstatite kugels with radiate structure are basic segregations out of a more acid magma as are the metallic forms out of the slag, nor indeed, to indicate that they result from the disintegration of previously solidified magmas of any kind.

II. With these facts in mind it seemed advisable to study the isolated chondrule, freed from its matrix. For this purpose the loose textured Allegan and Bjurböle stones, of pronounced tuffaceous type, afforded most excellent material, both in quality and quantity. From the disintegrated Allegan stone there were picked out a considerable series of the rounded granules such as are ordinarily classed as chondrules. These, under a hand lens, it was found, could be readily divided into two classes. First, those of regular spherical or oval form with comparatively smooth though lusterless surfaces, usually with one or more depressions as in figure 1, pl. 1, and sometimes with deeper pittings. These were of a light green color, in a few instances, but mainly dark, nearly black. When broken, such show a straight smooth fracture and distinct eccentric radial internal structure (fig. 2, pl. 1). Of a number of thin sections made none showed a holocrystalline or porphyritic structure. Some were so finely cryptocrystalline that it was only by analogy that they were determined as pyroxenic. Others were distinctly eccentric radiating, the bars possessing sufficient crystalline development to make it possible to determine their orthorhombic pyroxenic nature.

Second, the second series was composed of rounded and irregular granules with rougher surfaces, lacking the shell-like rind or casing of the first (Fig. 3, pl. 1). They were hard, undergoing no further disintegration under ordinary friction, sometimes quite spherical, but grading into angular fragments with angles blunted or rounded and with an uneven fracture. Sections of these showed a quite different structure from the first mentioned. Some were wholly crystalline, granular or of short radiating enstatites, while others were beautifully porphyritic consisting of olivine or pyroxene in a glass base. The porphyritic marginal crystals showed plainly that the present form of the granule was due to mechanical attrition (fig. 4, pl. 1). These forms are undoubtedly the ones that in the thin section give the forms shown in figures 8, p. 454, and 10 on p. 455. There can be no question but what they owe their present form and dimensions to attrition as suggested by Tschermak, though whether the

original forms were spheroidal or that of an ordinary angular fragment there is no absolute means of determining. From the examination of a large number of cases, I am inclined to the belief that these last are altogether of fragmental origin, broken fragments rounded by mechanical action.

From a lot of disintegrated Bjurböle material, generously furnished by Dr. J. J. Sederholm, there were in like manner picked out the two series shown in figures 5 and 8, pl. 1. It will be noted here that there is a greater contrast between the two forms than in the case of Allegan. Those in figure 5 are typical blebs of fiery rain, smooth, with a rind-like coating showing frequent depressions and more rarely excrescences. A large number of like forms were broken with a hammer and yielded smooth, cleavage-like fractures in part with a radiate structure. In thin sections all showed either a dense cryptocrystalline or radiate internal structure (figs. 6 and 7).

The forms shown in figure 8 were, as in the case of Allegan also, less symmetrically oval or spherical, rough exteriorly, never encrusted or showing depressions or excrescences.[29] On breaking with a hammer some of these yielded a smooth, straight, cleavage-like fracture and in the section showed a radiate structure; others and the larger part showed an uneven fracture and in the section were irregularly granular, glassy and porphyritic (figs. 9 and 10).

I find a similar, though not always so plainly marked, distinction in the chondrules separated out from the meteorites of Ochansk, San Emigdio and Soko Banjo. A further study of a large number of sections of chondritic stones fully confirms these observations. Stated in brief the conditions and conclusions are as follows:

(1) Only the chondrules of glass and cryptocrystalline or radiating enstatites (kugelchen) present the rounded or oval form with smooth rind-like crust and surfaces, with often one or more saucer-like depressions or excrescences such as are consistent with a theory of origin as fused drops of "fiery rain" (Sorby).

(2) Chondrules of a compound, holocrystalline nature, and those porphyritic through the development of olivine or pyroxene phenocrysts in a more or less glassy base are lacking in smooth exteriors and though often quite spherical in outline, are as a rule more or less irregular and in many instances show unmistakable evidences of an origin of form through mechanical attrition. These distinctions are well shown in figures 3 and 5, and in the general view from a thin section of the stone of Cedar, Texas (p. 462).

Many of the irregular forms, it is true, are found to be of the radiating or cryptocrystalline enstatite. This is evident from an examination of

the thin sections and has also been found to be the case in some of the forms freed from the matrix. They are plainly fragments of the encrusted forms and need no explanation. In no case, however, have I found a part glassy and porphyritic or holocrystalline structure developed in a polysomatic chondrule which does not suggest, from an examination of its external form, an origin as a rock fragment rather than a molten drop.

That all the forms described cannot be attributable to a common origin, I consider self-evident, as already noted. The objections to an acceptance of the "fiery rain," or "fused drop" hypothesis in accounting for these last forms may be concisely stated as follows:

(1) They are irregular in outline.

(2) They never show the smooth, rind-like surfaces so characteristic of the enstatite kugels.

(3) The outlines are not those of the original body from which they were derived, but for the most part plainly due to abrasion.

(4) They show no gradations in crystallization from border to center.

(5) They never show the peculiar surface depressions so characteristic of the enstatite radial and cryptocrystalline forms.

(6) In brief, their present structural peculiarities, both external and internal are entirely inconsistent with any conceivable theory of origin but that of detrital particles from solidified magmas.

It is perhaps questionable if such forms should be considered true chondrules and perhaps the term pseudochondrule or *chondroid* had best be applied to them.[30] The true chondrules are those of the spherulitic or *kugel* type; others are rock fragments reduced to their present form through mechanical attrition.

As bearing still further upon the question attention may be called to somewhat similar forms of known and artificial origin. In figures 11 to 13, pl. 1, are chondrule-like forms found among the iron furnace slags at the Sparrow Point, Md., works of the Bethlehem Steel Co. Such are certainly strongly suggestive, in form, to meteoric chondrules though it is to be remarked that they are, interiorly, vesicular, almost pumiceous. Like forms I have found being blown from the Bessemer converters in the more energetic periods of operation. They formed true drops of "fiery rain." In figure 12 is shown one of these imbedded in the scoriaceous slag. Qualitative tests show these to consist largely of silica and oxides of iron, a little lime, and included minute flecks of metallic iron.

In the above discussion, the fact has not been overlooked that very many of the glass and enstatite chondrules relegated above to the fiery rain hypothesis have likewise suffered abrasion through external agencies. This is plainly evident from the figures given and the multitude of fragmental forms found in many chondritic stones. The encrusted form of

so large a number of chondrules of this type and the complete absence of such crust on the porphyritic and holocrystalline forms I have just been discussing are in themselves sufficient to enable one to safely differentiate between the two.

The conclusions here reached are not quite so sweeping as those reached by Merrick in his humorous poem on the changeable colors of the chameleon, where he announces that "All are right, and all are wrong," but as is so frequently the case it seems to show that two or more of the opinions advanced were essentially correct and their apparent differences due to failure to recognize all of the facts I have here presented, and that of the many varietal forms of chondrules not all may have a similar origin.

Before leaving this section of the subject, it may be well, however, to present reasons for the non-acceptance of certain of the views presented and enumerated. It will be noted that the conclusions I have reached, bear out for the varying types the ideas of both Sorby and Tschermak, those of the first mentioned supported by the experimental work of Rinne. With the views of Borgström and of Ramsay, so far as they bear upon the subject in the papers quoted, I am in agreement. With those of Hussak, so far as relates to the production of chondritic structures through the corrosive action of a nickel-iron-rich magma, I find no confirmation. Not only is there little evidence that such a magma would corrode the silicates, but as in the case of meteorites of Brezina's Röckiky group (Admire and Eagle Station) there is direct evidence that it would not. Further than this, there is evidence that in my own opinion, tends to show that the iron of meteorites is altogether secondary and due to a reduction of some easily reducible compound like lawrencite, as I have elsewhere frequently stated.[31] The expressed conclusions of Berwerth and Wahl are not sufficiently detailed to admit of close comparison other than to say that they both seem applicable to the chondrules of the encrusted enstatite (kugel) type first mentioned, but not to the holocrystalline and porphyritic forms. With reference to Dufrenoy's comparison of this structure to perlitic glass[32] it can only be said that while at first thought it might seem applicable to the glassy and half glassy porphyritic types, I find nothing whatever to sustain it and several facts which militate against it. First there are in no instances indications of perlitic cracks and second, in the terrestrial perlites, so far as my own observation goes, the cracks always pass around any existing phenocrysts, rather than through them as they must to have produced the forms shown in figure 8. It may be added that perlitic structure in terrestrial rocks is regarded as due to shrinkage and limited to acidic magmas containing a considerable quantity of moisture. The structure is quite unknown in rocks as basic as are the meteorites.

With reference to the spherulitic nature of the chondrules as advocated

by Klein, I can only say that I find no evidence, with a single possible exception, satisfactory to me, to bear out the idea. This exception occurs in the stone of Barratta, Australia, in one slide of which I find the spherulitic bodies shown in figure 1, p. 451. These are as sharply outlined and as easily detached from the matrix as any spherulites and so closely resemble the true spherulite from one of the acid glasses as on casual inspection to strongly suggest their similar nature. Indeed, I have thus far failed to find evidence to completely satisfy me that such would not be so classed. It is to be noted, however, that between crossed nicols these break up each into several more or less well-defined areas showing an illy defined structure and over each of which the dark cloud sweeps irregularly as the stage is revolved. They lack the definite radial structure of typical spherulites and never between crossed nicols show the black cross, though how much reliance can be placed upon this distinction, I am not prepared to say. They are sufficiently distinct from the other chondrules in the stone to suggest an entirely different mineralogical nature, but the amount of material at my disposal is not sufficient to allow a determination. Liversidge,[33] in his description, makes no mention of these peculiar forms.

Finally with reference to Fermor's fused garnet theory it may be stated that in no case have garnets been proven to actually occur in meteorites, and further, did the chondrules so originate it would be a perfectly safe assumption that we should sometime and somewhere find residual traces. So far as my observation goes, no such cases are on record.

[1] Published by permission of the Secretary of the Smithsonian Institution. This paper is supplementary to the author's work on "Minor Constituents of Meteorites" made under a Grant from the J. Lawrence Smith Fund of the National Academy of Sciences (see Memoirs of the Academy, **14**, 1916, pp. 1–29; 1919, pp. 1–15; and these PROCEEDINGS, March and June 1915, April 1918, and January 1919).

[2] Geschreibung u. Eintheilung der Meteoriten, etc., *Abhand. d. K. Akad. Wiss. Berlin*, 1863.

[3] See Farrington, *Meteorites*, p. 200.

[4] Or "globular," see *Proc. Amer. Phil. Soc.*, **43**, 1904 (238).

[5] See particularly *Die Mikroscopische Beschaffenheit der Meteoriten*, and *Meteoritenkunde*, respectively.

[6] No attempt in these studies has been made to distinguish between enstatite and the ferruginous variety bronzite.

[7] *Pogg. Ann.*, **111**, p. 384.

[8] *Proc. Royal Soc.*, June 1864.

[9] *Nature, London*, April 5, 1877 (296).

[10] Die Trümmerstructur d. meteoriten, etc., *Sitz. k. Acad. Wiss., Wien*, **70**, 1874 (4).

[11] *Ibid.*, **71**, 1875.

[12] *Denk. Math. Natur. Classe kaiser. Akad Wiss*, **39**, 1878.

[13] *Sitz. k. k. Akad. Wiss., Wien*, **95**, 1882 (205).

[14] *Geologie Experimentale*, 1879 (530).

[15] *Neues Jahrbuch Min. Pet.*, **2**, 1895 (229–246).

[16] *Ibid.*, —, 1897 (259–61).

[17] *Centralblatt Min.*, etc., No. 21, pp. 641–47.

[18] Die Meteoriten von Hvittis u. Marjalatti, Helsingfors, 1903.

[19] *Trans. Royal Astr. Soc., Canada*, 1904.

[20] *C. R. Paris Acad. Sci.*, **96**, 1883 (868).

[21] *Die Meteoriten in Sammlungen*, etc., 1885.

[22] *N. Jahrb. Min.*, etc., **1**, 1884 (18–43). They immersed fragments of an "olivinefels" for many hours in a slowly cooling melt of nephelin basalt. The stone was strongly attacked and the outer portions, in close contact with the melt, shattered and corroded, the olivine granules becoming filled with embayments and enclosures of a secondary colorless glass, all strongly suggestive of meteoric chondrules.

[23] *Studien uber Meteoriten*, p. 35.

[24] *Zs. Anorg. Chem.*, **69**, 1910 (52–96).

[25] *Records Geol. Survey India*, **43**, 1913 (45).

[26] Such forms may readily be artificially reproduced by fusing to a bead some readily fusible mineral like stibnite. Although unfortunately opaque the radiate structure may be plainly seen on a broken surface of the globule.

[27] *Mikro. Beschaff. der Meteoriten*, plate 15.

[28] *Proc. U. S. Nat. Mus., Washington*, **44**, 1913 (326).

[29] Borgström and Ramsay in their description of the Bjurböle stone (*Bull.* No. **12**, *Com. Geologique Finland*) mention the irregularity in form of the chondrules, as seen in thin sections. They say, p. 18 under (6) *Polysomatische Olivinchondren mit Nickeleisen* "Die Chondren dieser Art haben selten regelmässig runde Form." and again, on p. 19 under (8) *Chondren aus Glas mit wohlbegrenzten dicken Olivinen*, "Sehr selten besitzen die Chondren von Typus (8) regelmassige rundliche Begrenzung, sondernerscheinen in den meisten Fallen mit Zufalligen eckigen oder buckligen Formen."

[30] In figure 1, of plate 8, of his *Mikroskopische Beschaffenheit*, Tschermak shows a small monosomatic chondrule inclosed in a larger porphyritic form of the same mineralogical composition, from the stone of Dhurmsala. It is to be noted, however, that while the smaller monosomatic form is evidently the direct product of a cooling molten magma, it has no necessary connection with the ground in which it is embedded and there is nothing in the appearance of the larger form, so far as can be judged from the figure, to indicate that it is not merely a rounded fragment of some pre-existing chondritic stone. This explanation will not, however, hold good for the double chondrule in the Borkut stone shown in figure 2 of his plate 19. In the case of our figure 3, plate 3, from the Okechobee stone there can be no question but that the chondrule is merely a fragment inclosing a large enstatite.

[31] I find no reason in the criticisms of Quirke (*Economic Geology*, **14**, 1919 (621)) to change my oft expressed views regarding the origin of the metallic iron through the reduction of lawrencite or some other easily reducible compound. Chlorine is a gas and in a dry atmosphere would readily escape of itself and without evident action on the silicate constituents. If means were to be "provided" it would have to be for the retention rather than for getting rid of this constituent.

[32] *C. R. Paris Acad. Sci.*, **13**, 1841 (48).

[33] *Jour. Proc. Roy. Soc., New South Wales*, **36**, 1902.

THE VERTICAL INTERFEROMETER

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Read before the Academy, April 26, 1920

The investigation of the distribution of intensity in spectral lines by interference methods while somewhat laborious as compared with the direct methods, visual or photographic, has the important advantage of freedom from instrumental uncertainties such as arise from optical imperfections,

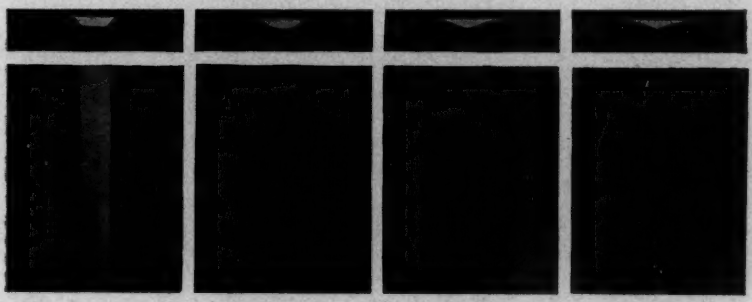

FIG. 1.

finite slit width and diffraction, which are especially objectionable in the case of lines or groups so narrow as to be barely within the resolving power of the spectroscopic appliances employed.

Besides the theoretical value of such investigations as affecting the interpretation of such distribution as based on various assumptions of kinetic theory, effect of temperature, pressure, electric and magnetic fields, a practical application may be mentioned, namely the accurate description of the appearance of spectral lines, which at present are characterized as broad, narrow, diffuse, etc. Thus, for symmetrical lines this distribution may be expressed by $y = 2^{-(x/\Delta)^n}$, in which Δ is the half width of the line and n determines the "square shouldered" character, as illustrated in figure 1.

One of the difficulties in the experimental determination of the visibility of the interference fringes as a function of the difference in path, from which the distribution of intensity may be inferred, is the maintenance of parallelism of the moving mirror, and the vertical interferometer is designed to obviate this objection.

For this the moving mirror is the level surface of mercury in a shallow dish (at least 7 cm. in diameter to avoid curvature due to capillarity),

The tremors of such a surface which would otherwise make observations impossible, are avoided by the simple device of amalgamating the

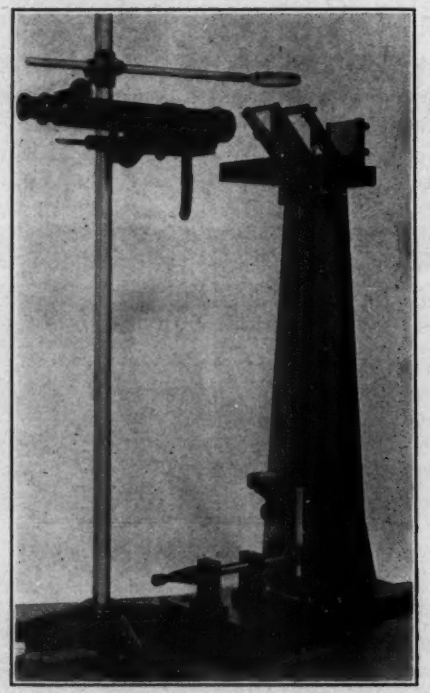

FIG. 2

surface of the shallow dish and covering with a layer of mercury a half millimeter or less in depth, whereby the vibrations are very rapidly extinguished. A photograph of the instrument is shown in figure 2.

ON THE APPLICATION OF INTERFERENCE METHODS TO ASTRONOMICAL MEASUREMENTS

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Read before the Academy, April 26, 1920

In order to test the practicability of using an interferometer with large base, for the purpose of measuring close double stars, and possibly the

diameter of some of the larger stars in the manner set forth in the *Philosophical Magazine* for July 1890, a trial was made at the Yerkes Observatory using two apertures in front of the 40-in. refractor.

The interference fringes at the focus remained fairly steady, with "seeing" only 2 to 3 on a scale of 5.

On invitation of Dr. George E. Hale the test was repeated with the same success with the 60-in. reflector.

With the 100-in. reflector the interference fringes were produced by a modification consisting in placing the screen with the two apertures at a short distance from the focus, which made the control of the distance between them and their position angle much easier. The interference fringes were again fairly steady, even when with "seeing" but 2 on a scale of 10, and consequently very considerable "boiling" of the images under the high magnification necessary (2000 to 3000 diameters).

The following is a resumé of observation of Capella by J. A. Anderson, showing that measurements may be obtained with an error of but a few ten-thousandths of a second of arc.

STATEMENT OF RESULTS FROM OBSERVATIONS OF CAPELLA WITH THE MICHELSON INTERFEROMETER APPARATUS

DATE OF OBSERVATION	POSITION ANGLE	DISTANCE
Dec. 30, 1919	148°.0	0".0418
Feb. 13, 1920	5.0	0.0458
Feb. 14, 1920	1.0	0.0451
Feb. 15, 1920	356.4	0.0443
Mar. 15, 1920	242.0	0.0505

The constants of the apparent ellipse fitting these observations are as follows:

Semi-major axis	0".0511
Semi-minor axis	0.0417
Position angle of major-axis	49°.2

The spectroscopic orbit being practically a circle ($e = 0.016$) the angle between the orbit plane, and the tangent plane to the celestial sphere is 35° (or better 145°, since motion is retrograde).

Parallax of Capella a little under 0".050.

Preparations are now under way at Mt. Wilson for testing the possibilities of the interferometer method with a base of 18 to 20 feet.

Further details will appear in the *Astrophysical Journal*.

A MODIFICATION OF THE REVOLVING MIRROR METHOD FOR MEASURING THE VELOCITY OF LIGHT

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Read before the Academy, April 26, 1920

The accuracy of any measurement of the velocity of light by the revolving mirror depends on that of three factors:

(1) The distance between stations.

(2) The speed of rotation.

(3) The angle described by the revolving mirror while light goes to the distant station and returns.

It is estimated that (1) and (2) may be determined to within one part in 200,000 and possibly one in a million.

The third measurement may be eliminated by constructing a revolving mirror in the form of a prism with a square or octagonal base, the angles of which may be made equal to 90° (or 135°) to this same order of accuracy.

For this a test angle is made by cementing a prism of approximately 90° (or of 45°) to a true plane. The square (or octagon) is applied to the two surfaces, producing interference bands when illuminated by the light from a mercury lamp.

If these are straight and parallel to the axis of rotation, the angles are equal; if not the difference, α, is measured by $\epsilon/2b$, if ϵ is the fraction of the fringe width and b the width of the mirror face. Thus if $\epsilon = \lambda/20$ and $b = 15$ mm., $\alpha = 0.000001$, which will, therefore, be the order of accuracy of the measurement under these conditions. For a wider face the accuracy would be still greater. If the angle of the test prism is θ_0 and the measured differences are α_0, $\alpha_1 \ldots \alpha_n$, then completing the 360° gives $\theta_0 = (360 - \Sigma\alpha)/n$, whence $\theta_1 = \theta_0 + \alpha_1$, $\theta_2 = \theta_0 + \alpha_2$, etc.

The faces are corrected until the angles are all equal within the limit of error assigned. This method of constructing reflecting surfaces inclined at equal angles, which may readily be extended to any number of surfaces, may prove useful in the construction of standard test angles, which may thus be made accurate to a tenth of a second or less; and also in the subdivision of meridian circles, etc., in which the limit of error is of the order of one or two-tenths of a second.

Such a revolving mirror has been constructed in form of an octagon with faces of 15 by 20 mm. and with air under pressure of one atmosphere,

a speed of revolution of over 1000 turns per second has been attained. This makes it possible to obtain a rotation of 45° while light is passing twice over the distance between stations $12^1/_2$ miles apart; or of 90° if the stations are 25 miles apart.

It might be thought that with so small a mirror face the intensity of the return image would be insufficient; but by placing the revolving mirror in the focus of a very long focus lens or mirror, and placing the slit source at a distance $v = f^2/D$, in which f is the focal length and D the distance between stations, the intensity is a maximum and yet a small mirror is sufficient to return all the light which enters the lens. This last (or the concave mirror which replaces it) should be as large as possible as also, of course, the distant mirror.

Incidentally the eight reflections from the revolving mirror, will give four times as much light as a two-faced plane mirror.

Preparations are now in progress for a preliminary test of the method at Mt. Wilson, where it is hoped a distance of 25 miles may be utilized.

ON THE K SERIES OF X-RAYS

BY WILLIAM DUANE AND WILHELM STENSTRÖM

JEFFERSON PHYSICAL LABORATORY, HARVARD UNIVERSITY

Communicated July 1, 1920

In the research reported in this paper the authors have measured the wave-lengths of the emission lines and that of the critical absorption in the K series of tungsten. In order to obtain as precise values of the wave-lengths as possible they employed spectra of the first, second, third, fourth and, in one instance, the fifth order.

The object of the research has been to provide data for testing the following points: (*a*) The existence of a third line in the α-group; (*b*) the separation of the critical absorption from the line of shortest wave-length in the emission spectrum, namely the γ line; (*c*) the experimental and theoretical relations between the various lines in the K, L, M, etc., series; (*d*) the relative intensities of the emission lines; and (*e*) the equations for the wave-lengths that may be deduced from theories of the structure of atoms and the mechanism of radiation.

The X-ray spectrometer, the generating plant used to operate the X-ray tube, the instruments for controlling the current and voltage and the general method of making the measurements have been described in papers from this laboratory published during the last few years in the

Physical Review. The chief difference between this and previous researches lies in fact that we have used spectra of higher orders, thus obtaining greater dispersion and precision.

Measurements of the electric current in the ionization chamber of the

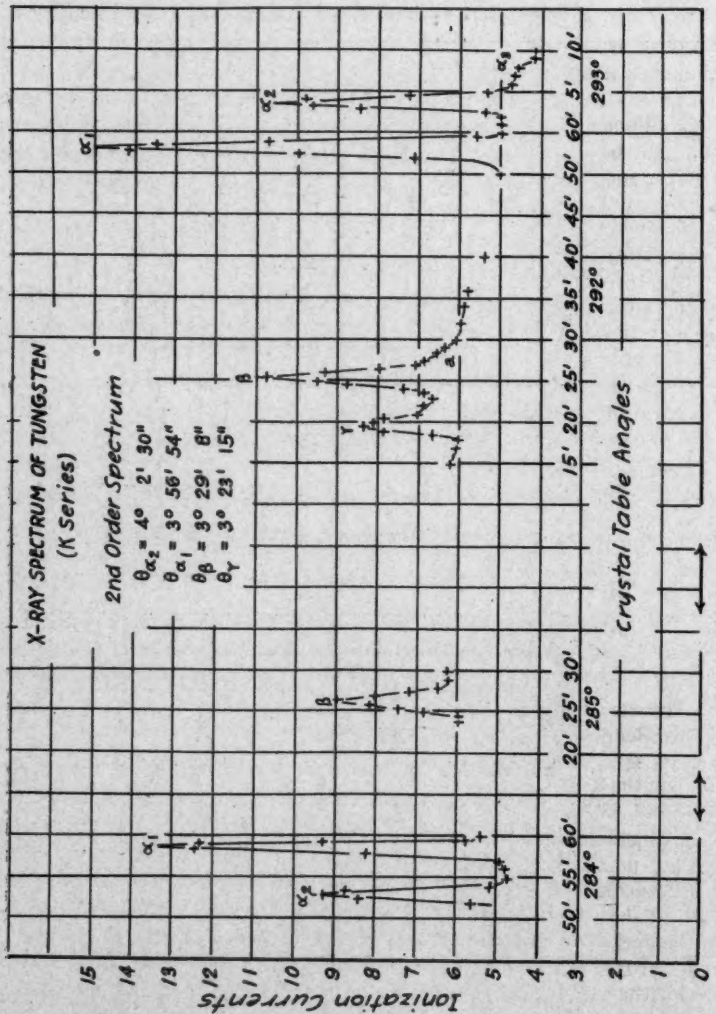

FIG. 1.

spectrometer were made at intervals of 30″ of arc on the scale which gives the grazing angle of incidence of the X-rays that strike the crystal. The curves in figures 1, 2 and 3 represent the ionization current as a function of the grazing angle of incidence, θ, in typical experiments.

The tips of the peaks in these curves correspond to the lines in the characteristic X-ray spectra. It is possible to determine with certainty between which two settings of the crystal a peak lies, and also about how far from each it is. This means that, when the apparatus is functioning well, the grazing angle of incidence can be estimated to within about 7″

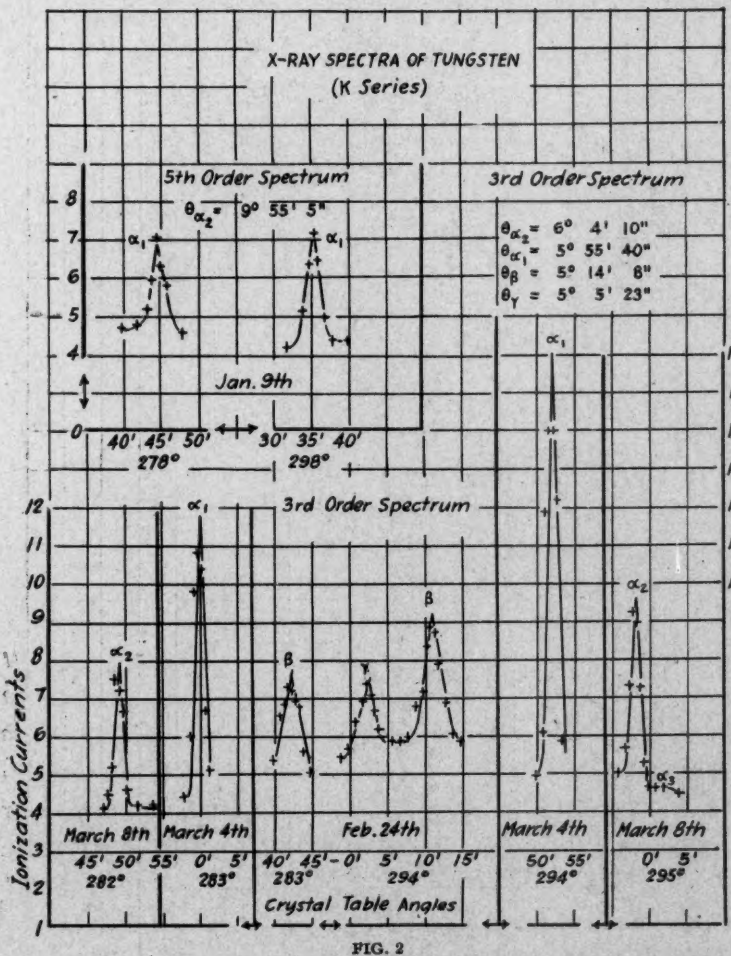

FIG. 2

of arc. For the tungsten K series the grazing angles of incidence in the first order spectrum amount to about 2° = 7200″, and, therefore, the error in a satisfactory determination of a wave-length ought not to exceed $^1/_{10}$, $^1/_{20}$, $^1/_{30}$, $^1/_{40}$, etc., per cent in the spectra of the first, second, third, fourth, etc., orders, respectively.

In estimating the wave-lengths, etc., only those measurements may be used that are made on the same date, for the zero of the instrument may have shifted between the dates recorded in the figures.

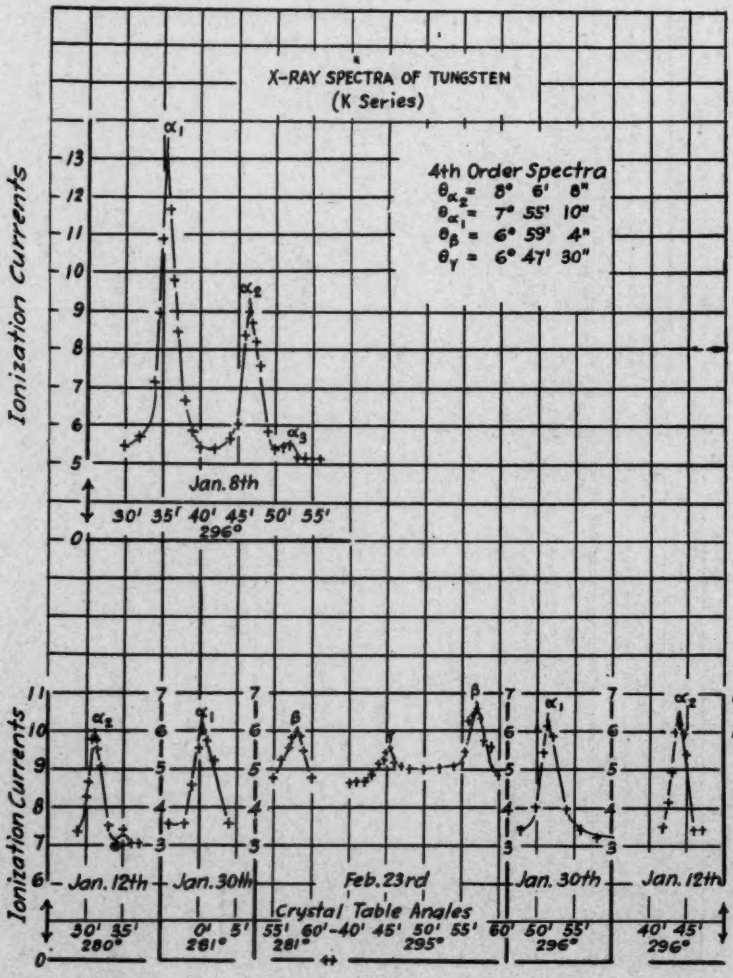

FIG. 3

The following table contains the wave-lengths, λ, calculated from the average grazing angles of incidence, θ, obtained from all the measurements we have made in the spectra of the various orders, respectively.

TABLE 1
K SERIES OF TUNGSTEN
$n\lambda = 6.056 \times \sin\theta \times 10^{-8}$ cm.

ORDER, n, OF SPECTRUM	$\lambda \times 10^8$ cm.					
	α_2	α_2	α_1	β	γ	ABSORPTION
1				0.18441	0.17898	0.17833
2		0.21342	0.20850	0.18415	0.17880	0.17810
3		0.21339	0.20848	0.18420	0.17909	0.17795
4	0.215	0.21341	0.20862	0.18418	0.17905	
5			0.20862			
Weighted Means	0.215±1	0.21341±3	0.20860±4	0.18420±3	0.17901±6	0.17806±7
Last Year		0.2134	0.2087	0.1842		0.1785
Siegbahn		0.21345	0.20878	0.18430	0.17934	
$\frac{1}{\lambda}$	4.65	4.6858	4.7938	5.4290	5.5863	5.6160
$\frac{\nu}{\nu_\infty}$	4239.	4270.3	4368.7	4947.4	5090.9	5118.0

The seventh line in the table contains the weighted mean values of the wave-lengths, together with a rough estimate of the precision of the measurements. The γ wave-length is more difficult to measure than the others owing to the fact that the critical absorption wave-length lies so near it. The target of the X-ray tube absorbs some of its own rays.

In order to estimate the absolute accuracy of the wave-lengths we must, of course, as in all work in X-ray spectra, take account of the errors in the grating constant of the reflecting crystal. These appear to add up to about 0.06%. (See a "Report on Data Relating to X-Ray Spectra," published by the National Research Council.)

For purposes of comparison we have added, in the eighth and ninth lines, respectively, the values obtained last year by the ionization method (*Physical Review*, July, 1919, p. 67) and those given by Siegbahn (*Phil. Mag.*, Nov., 1919, p. 639), which he measured by photographic methods. A small correction ($^1/_{1000}$) has been made in these wave-lengths corresponding to the value of the grating constant of calcite we use.

In order to test general relations between the wave-lengths and also theoretical equations it is convenient to have at hand the wave-numbers, $1/\lambda$, and the ratios, ν/ν_∞, of the frequencies, ν, to the fundamental Rydberg frequency, ν_∞. In calculating these ratios we have used the Rydberg fundamental wave-number, $\nu_\infty = 109737$, calculated for heavy atoms from the data obtained by Paschen in the spectra of hydrogen and helium. Line 10 in table 1 contains the values of $1/\lambda$, each divided by 10^8, and line 11 contains the values of ν/ν_∞, calculated from our values of λ.

The critical absorption wave-lengths appear in the last column of table

1. Dr. R. A. Patterson coöperated with us in making the measurements of these wave-lengths. We used an X-ray tube with a molybdenum target in these experiments.

It appears from the data that the wave-length of the γ line and that of the critical absorption differ from each other by about $^1/_2$ of 1%. In the K series spectrum of rhodium (see *Physical Review*, Oct., 1919, p. 369) the two wave-lengths differ from each other by about $^1/_4$%. If we adopt the theory of electrons revolving in atomic orbits we can explain this difference in wave-length as follows: According to the theory the critical absorption frequency equals the amount of energy required to carry an electron from the K orbit out to the periphery of the atom divided by Planck's constant, h. The γ line is due to electrons falling into the vacancies in the K orbit from orbits outside of those that we may class together and call the M orbits. The amount of energy set free and radiated during one of these transfers of an electron is less than the amount of energy required to lift the electron all the way from the K orbit to the periphery of the atom. Since the frequency of the X-ray emitted during the transfer equals this energy divided by h, it follows that the frequency must be less than that of the critical absorption, and, therefore, its wave-length greater than that of the critical absorption. The γ line is probably complex, and the wave-length we measure must be a kind of centre of gravity of the wave-lengths of its components.

In the second order spectrum (fig. 1) a drop occurs in the curve at a certain point marked "*a*." This does not belong to the K series of tungsten. It represents the critical ionization of iodine in the first order spectrum, the wave-length of which is 0.3737×10^{-8} cm. We used methyliodide in the ionization chamber of the spectrometer.

In a research described in the *Physical Review* for July, 1919, p. 67, Mr. Shimizu and one of us obtained experimental evidence showing that the difference between the K critical absorption frequency and any one of the L critical absorption frequencies equals the frequency of one of the emission lines in the K series. Theoretically this relation ought not to be exact, if orbits in some of the atoms are elliptical and in other atoms of the same chemical element, circular. The atoms with elliptical orbits contain amounts of energy that are different from the amounts of energy in atoms with circular orbits, and hence the change in energy when an electron is removed from the K orbit will not be the same in the two cases.[1] This means that the K critical absorption frequency should be complex, and that what we measure is a kind of average frequency. Strictly, according to the theory, the frequency difference law should apply to those atoms only that are exactly alike.

To test this point we have taken the absorption measurements made on the L series of tungsten by Dr. R. A. Patterson and one of us. The law applies, of course, to the wave-numbers as well as to the frequencies.

The wave-numbers of the three critical absorptions in the L series of tungsten are

$$La_1 = 0.8240 \pm 1 \quad La_2 = 0.9323 \pm 4 \quad La_3 = 0.9766 \pm 30,$$

each multiplied by 10^8. Subtracting these from the critical absorption wave-number in the K series and comparing the differences with the wave-numbers of the three α emission lines we get

WAVE-NUMBERS, $1/\lambda \times 10^{-8}$	
$Ka - La_1 = 4.792 \pm 0.003$	$K\alpha_1 = 4.7938 \pm 0.001$
$Ka - La_2 = 4.685 \pm 0.003$	$K\alpha_2 = 4.6858 \pm 0.0007$
$Ka - La_3 = 4.639 \pm 0.006$	$K\alpha_3 = 4.65 \pm 0.02$

It appears that in each case the difference between the K critical absorption wave-number and one of the L critical absorption wave-numbers equals the wave-number of one of the α emission lines to within the limits of error of the measurements. This agrees with the results obtained last year. If the above mentioned effect due to elliptic orbits exists at all, it appears to be too small to detect, with our present methods of measurements, in atoms of as high an atomic number as that of tungsten.

The existence of the third faint line, α_3, and the agreement of its wave-number with the difference between the Ka and La_3 wave-numbers is a point of some theoretical interest. According to Sommerfeld's theory of elliptic orbits the $K\alpha_1$ line is due to the transfer of an electron from the L orbit to the K orbit, if the L orbit is circular, and the $K\alpha_2$ line is due to a similar transfer, if the L orbit is elliptic. The difference between the wave-numbers of $K\alpha_1$ and $K\alpha_2$ calculated from the formulas he gives (*Atombau und Spektrallinien*, chapter 5) amounts to 0.1072×10^8, and this agrees very well with our experimental value for the same difference, namely, 0.1080×10^8. There appears to be no explanation on this theory for the third critical absorption, La_3, in the L series, nor for the third line $K\alpha_3$ in the K emission series. It would seem to be necessary to assume that there are at least two L orbits.

The peak corresponding to $K\alpha_3$ seems to be quite well marked on the curves representing spectra of higher order than the first, especially on that representing the fourth order. The line is very faint, having only about $^1/_{25}$ of the intensity of the $K\alpha_1$ line.

The $K\beta$ line is supposed to be due to electrons falling from the M orbit (or orbits) into the K orbit. We can not test the above frequency difference law in this case directly, for the critical absorption frequencies have not been measured in the M series of tungsten. Dr. Stenström, however, has observed three critical absorption wave-lengths in the M series of thorium and uranium. He has measured also the wave-lengths of emission lines in the M series of uranium, thorium and tungsten. Since the relative positions of the critical absorption and certain of the emission lines are about the same in the high frequency spectra of the different

chemical elements, we can estimate approximately where the critical absorption wave-lengths lie in the M series of tungsten. Making this estimate, and calculating the three critical absorption wave-numbers we obtain the following values, each multiplied by 10^8,

$$Ma_1 = 0.152 \quad Ma_2 = 0.157 \quad Ma_3 = 0.182.$$

Subtracting these from the K absorption wave-number, and comparing the differences with the Kβ emission wave-number we get

$Ka - Ma_1 = 5.464$
$Ka - Ma_2 = 5.450$ $\qquad K\beta = 5.429$
$Ka - Ma_3 = 5.434$

One of the differences between absorption wave-numbers, $Ka - Ma_3$ lies very close to that of the Kβ line. This suggests that the Kβ line is due chiefly to electrons falling from the third M orbit to the K orbit. If electrons also fell from the second M orbit to the K orbit they would produce a line on the short wave-length side of the principle Kβ line. Faint lines on the short wave-length sides of the Kβ lines have been observed in the spectra of a few of the chemical elements of low atomic number by Mr. Hjalmar working in Professor Siegbahn's laboratory.[3] The separation of these components of the Kβ line from each other amounts to a small fraction of 1%.

According to Rubinowicz' "Principle of Selection" transfers of electrons from the first M orbit to the K orbit ought not to occur.[3]

The faint component that has been observed by Siegbahn,[4] de Broglie[5] and Hjalmar on the long wave-length side of the Kβ line in the spectra of a few of the chemical elements was predicted by Sommerfeld[6] by reasoning from his theory of elliptic orbits.

These components of the Kβ line may account for some of the irregularities in the β peaks on our curves (figs. 2 and 3).

Some years ago Kossel suggested that the difference between the frequencies (or wave-numbers) of the Kβ and Kα lines should equal that of a line in the L series. It is well known that this relation, although approximately true, is not quite exact. An explanation for this discrepancy may be found in the supposition that the electrons producing the Kβ line may not come from the same M orbits as those producing the Lα and Lβ lines.[7] Dr. R. A. Patterson and one of us recently presented to the American Physical Society an account of some experiments which show that the wave-numbers of the $L\alpha_1$, $L\alpha_2$ and $L\beta_1$ lines of uranium and thorium equal the differences between certain of the L and M critical absorption wave-numbers. These relations may be expressed by the following equations:

$$La_1 - Ma_1 = L\alpha_1, \; La_1 - Ma_2 = L\alpha_2 \text{ and } La_2 - Ma_2 = L\beta_1.$$

In terms of the transfer of electrons between orbits these equations mean

that the $L\alpha_1$, $L\alpha_2$ and $L\beta_1$ lines are due to electrons falling from the M_1 and M_2 orbits into L orbits, whereas we inferred above that the $K\beta$ line was mainly due to electrons falling from the M_3 orbit into the K orbit. A rough quantitative test for this explanation may be obtained as follows: From the above three equations and the three equations representing the transfers of electrons producing the $K\alpha$ and $K\beta$ lines, namely

$$Ka - La_1 = K\alpha_1,\ Ka - La_2 = K\alpha_2 \text{ and } Ka - Ma_3 = K\beta,$$

we get the equations

$$K\alpha_1 + L\alpha_1 - K\beta = Ma_3 - Ma_1 \text{ and } K\alpha_2 + L\beta_1 - K\beta = Ma_3 - Ma_2.$$

The left-hand members of these equations have the values 0.043 and 0.039, respectively, for tungsten. The right-hand members, which are the differences between quantities that we have estimated by extrapolation, have the values 0.030 and 0.025, respectively. These small differences are of the same order of magnitude, which indicates that the explanation is approximately correct. We must remember, however, that the M series is probably more complicated than we have supposed. There are reasons for believing, for instance, that another critical absorption exists, of slightly higher frequency than Ma_3.

The ionization spectrometer furnishes us a method of approximately estimating the relative intensity of the lines in X-ray spectra. At present, however, we are not able to correct for the change with wave-length in the absorption of the rays by the target, the glass walls of the X-ray tube and ionization chamber, the reflecting crystal, etc. Nor do we know exactly how the coefficient of reflection of the crystal varies with the wave-length. It is not safe, therefore, to compare spectral lines that differ very much from each other in wave-length.

By estimating the heights of the peaks corresponding to the lines of the K series in the spectra of the first, second, third and fourth orders we have arrived at the following numbers representing approximately their relative intensities:

Line	α_3	α_2	α_1	β	γ
Intensity	4	50	100	35	15

It is interesting to note that, if we apply Sommerfeld's theory of the relative intensity of lines (Atombau und Spektrallinien, chapter 6) to the α_1 and α_2 lines, we get either 3 : 2, or 2 : 1, as the ratio of their intensities. Our experiments favor the latter ratio.

In an address delivered at the St. Louis meeting of the American Association for the Advancement of Science, last December (see *Science*, May 21, 1920, p. 505) one of us presented a set of calculations of the K critical absorption frequencies for a number of chemical elements. The calculations were based on Bohr's theory, with the additional assumption that the electrons were distributed among the orbits in much the same way

as they are distributed in the layers and shells of the Lewis-Langmuir static atom. The ratio, ν/ν_∞, of the K critical absorption frequency for tungsten to the Rydberg fundamental frequency is 5118 according to the theory, which is exactly our experimental value (see table 1). The agreement to four figures between the two values must be regarded as a matter of chance, however, as the errors in both the experimental and calculated numbers amount to a fraction of 1%.

[1] See Sommerfeld's paper on the fine structure of the β-line (*Münchener Akademie*, June 1, 1918 (367)).

[2] Professor Siegbahn kindly communicated these results by letter.

[3] *Physik. Zs.*, **19**, 1918 (441, 465).

[4] *Phil. Mag.*, June, 1919 (601).

[5] *Comptes Rendus*, May 25, 1920 (1245).

[6] *Sitz. Ber. Bayr. Akad. Wiss.*, June, 1918.

[7] Since this explanation was presented to the Physical Society in February, papers by W. Kossel and A. Sommerfeld (*Zeitschrift für Physik*, Jan., 1920) have reached us. In these papers the same explanation for the discrepancy is discussed as that here put forward.

STUDIES OF MAGNITUDE IN STAR CLUSTERS, XII. SUMMARY OF A PHOTOMETRIC INVESTIGATION OF THE GLOBULAR SYSTEM MESSIER 3[1]

By Harlow Shapley and Helen N. Davis

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Communicated by G. E. Hale, June 24, 1920

1. The study at Mount Wilson of the distances and structure of stellar clusters is of necessity based upon observations extensive in number and considerably varied in character. When general and comprehensive results have been obtained for a few typical systems, it becomes possible to interpret more accurately the less detailed observations on other clusters, and to place greater confidence in the hypothesis of the galactic system that has developed from the investigations of cluster and variable stars.

For two open clusters, Messier 67 and 11, and for two globular clusters, Messier 13 and 3, fairly complete surveys of the photographic and photovisual magnitudes of the individual stars have now been carried through and published in some detail; for about forty other systems the photometric and spectral investigations have been of a special character, and, in publishing the results, the observational data have for the most part only been summarized.

2. The catalogue of magnitudes and colors of 848 stars in Messier 3, which is appearing as *Mount Wilson Contribution* No. 176,[2] was begun in 1915. Fourteen photographs, made at the primary focus of the 60-inch telescope, have been used for the ten thousand measures of magnitude involved in the investigation. Special care was taken to look into the

accidental and systematic errors; the average probable error of a magnitude is about 0.04. Since the distance of Messier 3 appears to be known with higher accuracy than that of any other stellar cluster, we have in this photometric catalogue a useful collection of data on the *absolute*

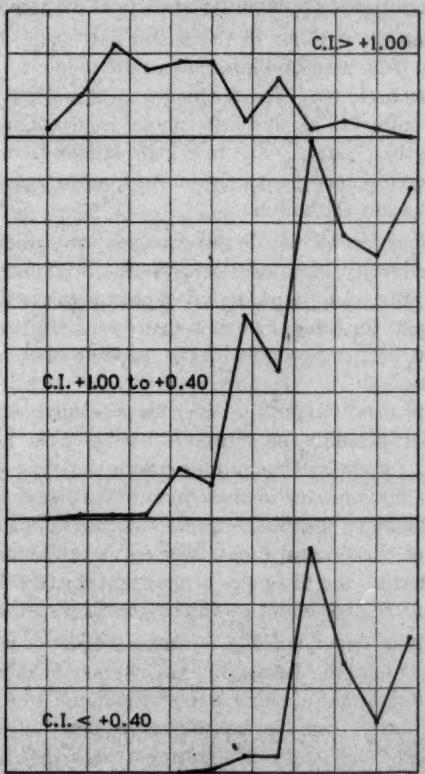

FIG. 1

Frequency of absolute photovisual magnitudes (abscissae) in Messier 3. Upper, middle, and lower curves are for red, yellow, and blue stars, respectively. Ordinates are numbers of stars for intervals of 0.4 in absolute magnitude.

brightness of stars—data that will become still more valuable as spectral observations are extended.

3. Von Zeipel's catalogue of positions[3] is used as the basis of the system of coördinates and of the system of numbering. For 340 stars fainter than the limit of his catalogue approximate positions have been determined from Mount Wilson plates.

The magnitudes of stars within 1'.8 of the center have not been mea-

sured. Outside this crowded central region the catalogue includes practically all cluster stars to the apparent photovisual magnitude 16.8, and it is nearly complete to the seventeenth magnitude. Excluding questionable values, and also results for 64 stars fainter than pv. mag. 17, we have 750 magnitudes available for statistical discussion.

4. By accepting the parallax of the cluster as 0".000072 (*Mount Wilson Contribution* No. 152), zero absolute magnitude is found to correspond to apparent magnitude 15.72. The frequency of stars may be determined throughout the interval of absolute photovisual magnitude from the brightest star in the cluster, −3.5, to a little fainter than +1.0. All the stars studied, therefore, are giants, the faintest ones catalogued being some twenty times as bright as the sun.

5. The frequency curves of absolute magnitude, which are graphs of the so-called luminosity law, are shown in the accompanying figure for red, yellow, and blue stars, separately. The curves are comparable with the combined results for three clusters in figure 4 of the preceding communication of the series,[4] except that in the present case a fainter magnitude is reached.

The preliminary maximum in the curve for blue stars (and less distinctly for yellow stars) at absolute magnitude −0.3 shows the highly significant excess of stars at the median magnitude for cluster-type variables.[5] The variety and lack of symmetry in these luminosity curves, and the certain existence of preliminary maxima, suggest the impracticability of using a small fragment of the general luminosity curve (all colors combined) as a means of estimating the distances of globular clusters—a procedure recently followed by Schouten and by Coeberg with surprizing results.

6. No conspicuous dependence of mean brightness or color on distance from the centre is found in Messier 3. The characteristic relation of mean color to absolute brightness appears, much as in figure 3 of the communication referred to above; and the maximum frequency of color index falls between +0.4 and +0.8, quite in agreement with the earlier results for Messier 13.

7. A separate investigation of the color curves and the constancy of period is under way for the numerous Cepheid variables in Messier 3. It will include an examination of the small light variations that appear in a number of stars very similar in color and absolute brightness to the typical cluster-type Cepheid, but not heretofore recognized as variable.

8. The apparent visual magnitude of the cluster as a whole is 6.6. The integrated absolute magnitude is, therefore, 6.6−15.7 = −9.1, which is equivalent to 360,000 times the light emission of the sun, or to a total energy of radiation of 1.4×10^{39} ergs a second.

[1] Messier 3 = G. C. 3636 = N. G. C. 5272; R. A. = $13^h\ 37^m\ 35^s$, Decl. = +28° 52′ 56″, 1900; galactic longitude = 8°, galactic latitude = +77°.

[3] Shapley, Harlow and Davis, Helen N., *Astrophys. J., Chicago, Ill.*, **51**, 1920 (140–178).

[3] Von Zeipel, H., *Annales de l'Observatoire de Paris, Memoires*, **25**, 1908 (F1–F101).

[4] Shapley, Harlow, These PROCEEDINGS, **6**, 1920 (293–300).

[5] *Ibid., Mt. Wilson Contribution* No. 155, 1917 (1–12); No. 175, 1919 (1–13), p. 10.

THE INFLUENCE OF ENVIRONMENT UPON DEVELOPMENT

BY FRANZ BOAS

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Read before the Academy, April 27, 1920

During the last ten years I have carried on a number of investigations on the relative influences of heredity and environment upon human types. On the whole the results show that each hereditary type can be considered as stable only in a stable environment, and that with a change of environment, many of the characteristic features of the body undergo changes. These results have been corroborated later on by investigations on immigrants in Boston and in certain respects also by Dr. Hrdlička's observations on Americans whose ancestors have been residents of this continent for several generations.

Ever since Gould and Baxter's investigations on the soldiers enlisted during the war of the Rebellion and since Bowditch's investigations of Boston school children, it has been known that the bulk of the body differs not only according to descent, but also according to social position of the child. The children of the poor are not so tall as the children of the well-to-do. We have, however, never been able to establish definitely whether children who are small for their ages will also be small as adults, or whether a compensatory growth occurs during the period of adolescence. I have shown in previous investigations that children who are retarded will at a later time have a much more rapid growth than children who in the early years of their lives are accelerated. This observation has been confirmed by a study of material which I placed at the disposal of Dr. Wissler. At that time, however, we were not in a position to extend these observations up to the adult stage.

Through the courtesy of Mr. Wilson Farrand, Headmaster of the Newark Academy, I have been able to collect observations on about 120 individuals whose measurements extend from the 11th year to adult life. The study of this series shows conclusively that from 11 years on, a short child will also be on the average short as an adult, while a tall child will be tall as an adult. The absolute amount of increment for a short child, from the age of 11 years up is, however, considerably larger than the total increment for a tall child of the same age, so that the previous observation may be extended for the whole period of growth. It seems likely that the relation between initial stature and increment will be

much slighter for earlier years, but no material is available relating to this question. A more detailed study of the relation between increment and stature shows that the increment cannot be represented as proportional to the deviation of stature from the norm, but that a more complex relation prevails. This is evidently due to the fact that at a given age there are, among short individuals, a considerable number who are retarded in development and that the number of those retarded decreases rapidly with the increase of stature so that among tall children of a given age those of accelerated development prevail. Since the increments of stature from ten years on to the adult stage cannot be expressed by a linear equation dependent upon age, it follows that the correlation between increment and adult stature as dependent upon initial stature can also not be expressed by a linear equation.

In other words we have to consider two factors. On the one hand there is the effect of retardation and acceleration which is most prominent in children of low and of high statures respectively and which results in a high increment for short children and a low increment for tall children and on the other hand there is the effect of hereditary stature which results in a low increment for short children of normal rate of development and in a high increment for tall children of normal rate of development.

It would be unreasonable to assume that among individuals who by heredity would grow to be tall, there should be a few only who would be retarded; since the causes of retardation are present in the whole socially uniform group which has been studied. The children are all children of well-to-do parents, well-nourished and living in a fairly open country, attending a school in which their health is carefully looked after. The retardation must, therefore, be due partly to an hereditary determination of the period of development; partly to individual pathological or semi-pathological conditions, which are not likely to be dependent upon hereditary stature. If then we find that those individuals who are short as adults, show from 11 and 12 years on a considerable degree of retardation, this can only be explained by the fact that the retardation has a permanent effect upon the development of the bulk of the body. Considering these conditions we must conclude that the loss of stature due to retardation in early childhood is not made up during the period of adolescence. In other words, that the ultimate stature is an effect of both hereditary and environmental causes.

I have investigated the same question with a different kind of material. I had an opportunity to examine a large series of Jewish children in New York City, living under different conditions. One group were students in a private school, the children of well to do parents; the second lived under normal family conditions on the East Side of New York City; the third group were boarded children under the care of charity organizations; the fourth group were placed in an educational institution in the open

country and organized on the cottage system, being placed in houses in groups of 30 to a house. The fifth group were children who were brought up in large institutions located in the city each institution containing more than 1000 children. A comparison of the children in the private school with all the other groups brought out very great differences. I have not been able to obtain adequate material covering the whole period of growth, but at the ages from 11–13 years the differences in both males and females are very considerable indeed. Judging from the observations made by Jacobs in London, it is safe to assume that these differences would be found to continue up to the adult stage.

In groups of children under public care continuous records were available and a comparison of the measurements of the various series of children measured on the date of commitment to public care showed that these series and that of children in normal East Side homes are strictly comparable. The statures and weights of individuals committed to a large institution, to an institution run on the cottage system and those placed in families under the care of charity organizations equaled the development of normal children from the East Side. When, however, the development of children in each group was examined, it was found that the institutionalized children lagged behind regularly. There is no doubt that the general conditions of nutrition in the large institutions, are as good as may be expected, but the whole institutional atmosphere has the effect of retarding development. Examination of adults who have passed through the asylums showed a similar effect. The average stature of adult men and women belonging to this group is about 2 cm. less than that of normal individuals of the East Side. The results obtained in an institution run on the cottage system are more favorable. While the average statures and weights are not quite up to normal, the differences are very slight. Children who are boarded in families, are in every respect comparable to the normal Jewish child in East Side families. There is only one exception to this rule. Children who are excluded from one of the institutions referred to and who are boarded in families are considerably less favorably developed than institutional children. This, however, is obviously due to the method of selection, because only children with pronounced pathological conditions are boarded out by the institution in question.

I was interested in the question whether the differences in development are due to physiological retardation and, partly for this reason, partly on account of our lack of knowledge of conditions of dentition, I collected extensive data on the eruption of teeth among the children here referred to. The observations were made by Dr. Milo Hellman to whose interest in the investigation I am greatly indebted. I have calculated before from data at hand what seems the most probable time of eruption for different teeth. In 1915, I collected similar material in Porto Rico

which was elaborated by Mr. Leslie Spier. Other material was placed at my disposal by Dr. Carl Roese of Dresden, whose investigations on the relation between anthropological form and the condition of teeth are of fundamental importance, and finally I used a large series of casts found in the offices of orthodontists, for which series I am obliged to the good offices of Dr. Hellman. The series previously published by me suggested that the difference in period which characterizes the development of the two sexes, is not found in the development of the teeth. As I have pointed out before, the development of the head shows, that a sexual difference amounting to about $1^1/_2$ years in the rate of physiological development occurs about the fifth year, and we know that this difference increases to a little more than two years at the time of maturity. The time of the eruption of the permanent canines and of the premolars is between the tenth and eleventh years. During this time the normal difference in regard to physiological development between the two sexes is approximately a year and one-half, perhaps a little more. For the teeth, however, we obtain a difference of less than $^1/_2$ a year, which shows clearly that the dentition of males and of females must possess definite secondary sexual characteristics. An investigation of the size of the teeth does not bring out any very striking differences. E. Mühlreiter (*Anatomie des menschlichen Gebisses*, Leipzig 1891, pp. 128, 129) in his investigations of the teeth, has suggested that there are characteristic differences in form. Further investigations, however, will be necessary in order to show whether the physiological difference in development is accompanied by a morphological difference in form.

I expected that, if there should be physical retardation, we should find among the children of the well-to-do an early development of the permanent teeth and among the children of the poor, a later development. However, just the reverse was found. The eruption of permanent teeth among institutionalized children showed an acceleration of approximately eight months. I think this puzzling phenomenon may be explained by the fact that in the dental care of institutional children there is a strong tendency to remove the deciduous teeth as soon as they show decay, and that the removal of the teeth acts as a stimulus upon the development of the permanent teeth. Possibly the conditions in Porto Rico, where we also find an unusually early eruption of permanent teeth combined with a marked retardation in the development of stature, may be explained in a similar way; namely, by early decay of deciduous teeth which brings about an early loss of the teeth. I am still engaged in an investigation of the eruption of the deciduous teeth in relation to bulk of body and I hope to report on this subject at a later time.

The evidence that may be derived from a study of stature and weight alone indicates that the under-development of the children in charitable institutions is due to retardation.

On the whole the observations here reported confirm the conclusion that environmental conditions play an important part in the determination of the bodily form of the adult. It is obvious, therefore, that explanations that take into account only hereditary causes cannot satisfactorily account for the observed phenomena. This is particularly true of stature and weight, which appear extremely variable in the same lines of descent, according to the environmental conditions in which the individuals live.

THE POSSIBILITIES OF THE ROCKET IN WEATHER FORECASTING

BY ROBERT H. GODDARD

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Communicated by C. G. Abbot. Read before the Academy, April 27, 1920

Most Desirable Conditions for Obtaining High Altitude Data for Weather Forecasting.—It is well understood that the pressure, temperature, wind velocity, and moisture content, which obtain at the top of the troposphere, i.e., at the 10 km. level, would be of much importance in weather forecasting; making possible the prediction of surface conditions many miles distant from the place of observation.

The data would obviously be of greatest value if obtained simultaneously at a number of separated stations. If this were done, an accurate weather map, representing conditions at a definite high elevation could be made, and compared with that representing surface conditions. Such a high altitude weather map would also be of obvious importance in aviation.

It is evident that the time of ascent should be as short as possible, not only in order that the data for the various stations should be obtained simultaneously, but also in order that drifting by the wind, and the consequent difficulty of recovery of the apparatus, be reduced to a minimum. The descent should also be as rapid as possible, for the same reasons, with proper arrangements to prevent damage on landing.

It would also be desirable, although not essential, that the instruments remain at, or near, the 10 km. level for from one to five minutes.

In short, then, the most desirable method of obtaining high altitude data for weather forecasting, would consist in the sending of instruments to the 10 km. level daily, from a number of stations, the ascent and descent being as rapid as practicable; provision further being made, if desirable, for maintaining the instruments at this level during an appreciable interval of time.

The Rocket Method as a Means of Realizing These Ideal Conditions.—Although the ordinary rocket has a vertical range of but a few hundred

feet, the rocket should, in principle, be capable of reaching much greater altitudes.

A theory, together with estimates as to what should be attainable under realizable conditions, is given in *Smithsonian Miscellaneous Collections*, Vol. 71, No. 2. It is intuitively evident, however, that a great elevation must be obtainable provided a large part of the heat energy of the propellant is converted into kinetic energy of the ejected gases, and also provided the proportion of mass of propellant to total mass is high.

It should be understood that although theory indicates the possibility of reaching great altitudes, the application discussed in the present paper is solely the raising of recording instruments to a moderate height.

As regards the conditions to be satisfied by the most desirable method above outlined, other than the mere attainment of the altitude, the rocket method is ideally suited to the raising of apparatus rapidly and without jar, inasmuch as the propulsive force is sensibly constant, and, therefore, the apparatus, starting from rest, will rapidly attain a high velocity. After the propellant has been completely expelled, the apparatus will gradually be brought to rest by gravity. A parachute device, carried by the rocket, could permit of any desired speed of descent; the details of construction and operation being, of course, well understood.

Concerning the maintenance of high level for an appreciable interval of time, the rocket apparatus can contain, besides instruments and parachute, a rubber balloon of the usual size, compactly folded, together with a steel sphere containing compressed hydrogen, to be released into the balloon when the upper limit of flight has been reached, as suggested in principle by Dr. Abbot. The parachute would be brought into action, and the balloon released, a predetermined interval of time after the balloon had been inflated.

Extent to Which the Conditions Necessary for a Satisfactory Rocket Method have been Realized.—The first condition for increasing the range of the rocket apparatus is the ejection of the powder gases with as high a velocity as possible. This feature has already been developed to a satisfactory degree. The velocity of the ejected gases has been increased from 1000 ft./sec. to over 7,500 ft./sec.; or, expressed differently, the fraction of the heat energy of the powder that is transformed into energy of motion of the ejected gases has been increased from one-fiftieth to over a half; the propellant being a dense smokeless powder. It is important that the velocity of ejection be large, as it enters exponentially in the expression for the initial mass necessary to propel a rocket to any given height.

Regarding the second condition for a great range, namely, the employment of a large proportion of weight of propellant to total weight, it should be understood that this proportion need not be large for a range as low as 10 km. It is necessary to employ the principle of multiple charges, as explained in the above Smithsonian publication, if the pro-

portion is to exceed the value for ordinary rockets, which is about $^1/_5$. This action consists in the loading and firing of a number of charges successively in the same combustion chamber.

The results of work upon this feature, to date, have been the development and experimental demonstration of a simple and light multiple charge apparatus, firing a few cartridges and travelling straight. In order to complete the development, it is necessary to adapt the apparatus to fire a large number of cartridges, and to make the parts, exclusive of propellant, sufficiently light. Work on increasing the number of cartridges is in progress.

In order to complete the development with a minimum of expense, the perfecting of details should be carried out only insofar as is necessary in order to produce an inexpensive apparatus. The only expense of maintenance will be a new magazine for each ascent.

In any case, the time required to reach the 10 km. level should be of the order of 20 seconds, if the retardation due to air resistance and gravity is minimized.

As an illustration of what should be possible with an apparatus developed in this way, it may be said that, using as a basis for the estimate a velocity of ejection of 5,500 ft./sec., which is easily obtained, a rocket weighing of the order of 11 lbs. initially and 6 lbs. at the highest point would be needed in order to send instruments weighing one pound to the 10 km. level.

NOTE ON A PNEUMATIC METHOD OF MEASURING VARIATIONS OF THE ACCELERATION OF GRAVITY[1]

BY CARL BARUS

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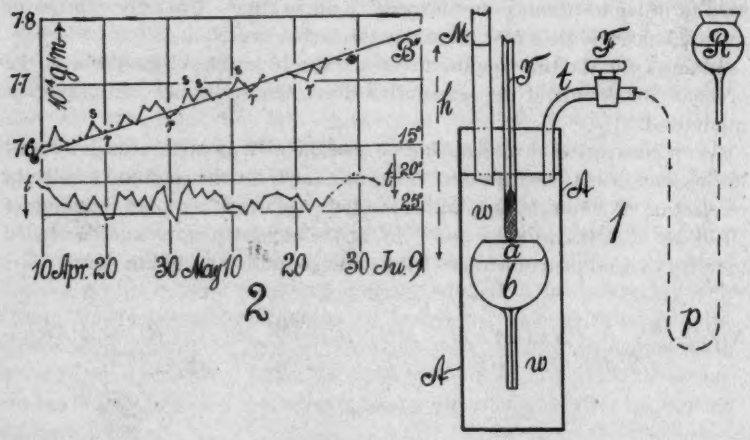
Communicated June 22, 1920

1. *Introductory.*—Some years ago I made an extended series of experiments[2] on the diffusion of gases through water; the gas in this work was imprisoned in a cartesian diver, and the very sensitive conditions under which the diver floats at a given level were made the criterion of measurement.

Inasmuch as such experiments consist virtually of a comparison of weights with the forces derived from air pressures, it must, therefore, be possible to obtain the acceleration of gravity in terms of these pressures, just as in another place I shall describe interferometer experiments made to evaluate the changes of g in terms of torsion. It will not, of course, be possible to determine g absolutely in this way because so little air is used; but it was hoped that the changes of g, in a proper environment would be determinable with some precision. Quite apart from this specific purpose, however, the long range experimental data are of varied interest.

2. *Apparatus.*—The chief difficulty with the experiment at the outset seems to be the elimination of loss of gas by diffusion. This, in the case of a cylindrical diver, open below, amounted in the experiments cited, to 0.41% per day. It would not, therefore, be long before the whole of the gas would be lost. To avoid this, two resources suggest themselves: (1) to place the diver about midway between two layers of air, one below and the other above it; and (2) to provide the diver with a long slender neck below, precisely what was to be avoided in cases of diffusion. I shall not describe the forms of apparatus in which the first precautions were taken, as they proved to be unavailable; nor any of the correlative forms with the diver sheathed within a completely submerged cylindrical tube, nearly closed. The results so obtained were no better than those



FIGS. 1 AND 2

found with the extremely simple apparatus, figure 1. The swimmer being here lighter than water, AA is the standglass, wholly filled and a, b, the swimmer held down at a definite level by the thermometer T. M is a water manometer for registering the pressure excess at which flotation just occurs at the given level. Pressure is applied by aid of the small water reservoir, R, communicating with AA by the tube t with the stopcock F, and the flexible pipe p, the whole system $t\ F\ p\ R$ being full of water. To make an observation, R is raised sufficiently and clamped. The cock F, is now slightly opened, until the swimmer just begins to sink. The head of water, h, is then read off at M. This adjustment may be made to a fraction of a millimeter of water. The apparatus admits of observation in an agitated environment.

The diver, which to be sensitive must be as light as possible (thin stem preferable), consisted of a bulb 4.5 cm. in diameter and a stem 4 cm. long and about 2 mm. in diameter. The rate of loss would be decreased

by diminishing this diameter further; but it is by no means proportional to the sectional area.

3. *Equations.*—If M is the mass of the diver, m that of the air contained at the absolute temperature τ and pressure $h + (H - \pi)\rho_m/\rho_w$ in centimeters of water (h being the incidental water head, figure 1, H the residual mercury head, and π the vapor pressure in centimeters of mercury, ρ_m, ρ_w, ρ_g, the densities of mercury, water and glass, all read off at τ), we may write for the gravitational acceleration, g:

$$g = \frac{Rm\tau}{[h + (H - \pi)\rho_m/\rho_w][(1 - \rho_w/\rho_g)M + m]}. \qquad (1)$$

The first term in the denominator is to be multiplied by ρ_w at τ, to get the water head at 4° C.; the second term to be divided by ρ_w to get the volume of the air bubble. These reductions, therefore, cancel each other. Mercury, water, and air are necessarily at the same temperature, at least in a proper installation of apparatus.

Thus it follows that variations of g are determinable with the same accuracy as τ and H. The former being of the order of 290°, 0.03° C., must be guaranteed if g is to be correct to 10^{-4}. Furthermore, H, which is over 1000 cm. of water, must be read to 1 millimeter for the same accuracy. Both of these requirements are moderate. By tapping the tables from below, the swimmer is slightly separated from the thermometer above, and the degree of flotation may thus be estimated with great precision, by the speed with which the swimmer rises again into contact. An accuracy of 0.01% in g may thus be expected, provided m is adequately constant. This paper indicates to what degree this may be insured; for in a heated room, in winter, with very variable temperatures, the test is extremely severe.

The absolute determination of g is dependent on the value of m. To measure this large swimmers would be needed such as would make the apparatus too cumbersome. With regard to small quantities or small changes of the quantities ρ_m, ρ_w, etc., equation (1) may be stripped of its less important terms and written in an approximate form. The corrections are then found by logarithmic differentiation.

4. *Observations.*—Figure 2, curve B, contains the most complete series of observations made with this apparatus, g/m in ten thousands, being shown in its variations within a lapse of somewhat less than two months. Readings were made about at noon daily and the corresponding temperatures are given (increasing downwards) in the graph marked t. Corrections for the temperatures, etc., have been applied. As the air content was too small for persistent flotation, the diver was liable to sink at low temperatures, or high barometers, or both (s, in the figure). It is under these circumstances that the irregularities of the curve are usually marked.

In the figure, a line has been drawn through a succession of points which seem to suggest it. For this g/m increases per day (owing to the decrease of m) at a mean rate of $r = 0.00039$, which is equivalent to and air loss of 5×10^{-6} grams per day. Though it was formerly my opinion that the phenomenon could be completely explained in terms of the diffusion of air through water, the cusps (s) and subsequent intervals of relapse (r) when temperature increases, again do not bear this out. The whole is more complicated. The mechanism by which the air loss is brought about is largely solutional. Air is dissolved when temperature falls and is released from solution when temperature rises again. But for this the rate of increase of g/m throughout many months is in all the results remarkably constant and could be allowed for. Only at the beginning of the experiments with a new diver, and owing probably to gas adhering to the glass walls, are these mean rates irregular.

But the solutional effect in question under rapidly varying temperatures is fatal to the purpose for which the experiments were made. Though much of the discrepancy could be removed by placing the whole apparatus in an adequately constant thermostat, the outlook is not encouraging.

[1] Advance note from a Report to the Carnegie Institution of Washington, D. C. The work was done in deference to a request of President R. S. Woodward, that a variety of methods of the kind in question be looked into.

[2] *Carnegie Publications*, No. 185, Washington, 1913.

NOTE ON TORSIONAL MEASUREMENT OF VARIATIONS OF THE ACCELERATION OF GRAVITY BY INTERFERENCE METHODS[1]

BY CARL BARUS

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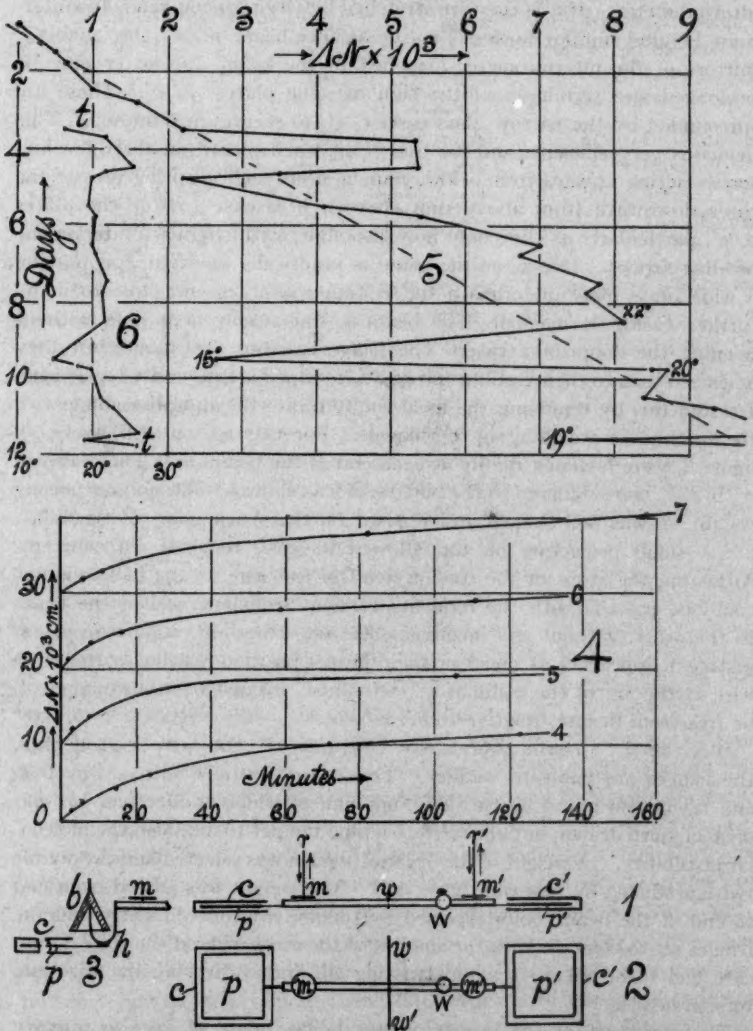
Communicated July 1, 1920

1. *Apparatus.*—These experiments were undertaken to find in how far interferometer methods might contribute to the measurement of changes of g, under conditions in which the pendulum is inapplicable. The apparatus as a whole is a horizontal torsion balance with the deflection readable in terms of the displacement of the achromatic interference fringes. The method developed would be applicable in case of any ordinary chemical balance. The interferometer described admits of moderately rough handling; but it requires a level base, at least during observation. The mirrors are adjusted normally to a vertical plane, as the rays are in this direction.

Torsional weighing, by passing the counterpoise from pan to pan, is in itself a delicate operation when fringes are used. Moreover, the variation of viscosity is particularly large at the beginning. Nevertheless

my first observations were made with such an apparatus; and adequate air damping was secured by hanging the light scale pans in dash pots. Readings were made on a large graduated circle (torsion head), the stops of



which were micrometer tangent screws. Changes of twist registered by the interference fringes were evaluated either by the latter, or by the micrometer screw of the rectangular interferometer.

If the weight remains on one side, viscous deformation not only grows

indefinitely smaller and is subject to simpler equational conditions, but the apparatus itself may be made less cumbersome and steadier. Thus in figures 1 (elevation) and 2 (plan), r and r' are the two light rays of the interferometer. If w is the wire stretched between torsion heads (counterpoise W) and running normally to the balance beam, m, m', the auxiliary mirrors of the interferometer attached to the beam, the latter may be prolonged and terminate in the thin metallic plates, p, p'. These are surrounded by the narrow glass cases c, c', to secure air damping. This device is very efficient; and if c and c' are each placed on three leveling screws acting upward from below, while a stout vertical spring presses the cases downward from above (not shown), it is easy to free the plates p, p', particularly as they may now also slide, with slight friction, on the leveling screws. Hence, as the beam is practically aperiodic, on putting a wide brass plate underneath the system $p\ m\ m'\ p'$ and close to it, no further casing is needed. The beam is thus easily accessible without opening the doors of a case. The fringes occasionally move, but they at once return to their equilibrium position, which is thus easily recognized. I tested this by removing the fixed counterpoise W, and proceeding with the method of weighing by exchanges. For this reason stiff hooks, h, figure 3, were fastened rigidly at each end of the beam, m. The plates p, p', in c, c', were soldered to the bottom of h as shown. The counterpoising weight, b, was bell shaped and reposed on the sharp point of the hook, h. A small projection on top allowed its easy removal with forceps. Adjusting the stops of the torsion head of the wire w, the bell weight b could be passed (with the requisite torsion) from one end of the beam to the other, without any inconvenience, the fringes in either case of adjustment appearing at once, in their proper position relative to the cross wire at the slit of the collimator. Of course, h must be stout enough to be free from flexure, relative to b.

Observations. Twists Alternately Opposite.—If the wire is too thin, the fringes are liable to wander. This ceases entirely with a wire 0.05 cm. thick, and about 35 cm. long, one-half of which is effective. It was of steel, hard drawn, but annealed to a blue temper to diminish the molecular instability. A weight of the order of 1 gram was selected and the double twist produced by this was $\Delta\theta = 50°$. The weight was passed from end to end of the beam, counterpoised by torsion, without difficulty and the fringes set to coincide with the shadow of the cross wire on the slit. Great care had to be taken to avoid twisting the frame carrying the wire and torsion heads.

The constants of the interferometer being, angle of rays to mirrors, $i = 45°$, breadth of the ray parallogram, $b = 10$ cm., micrometer displacement ΔN, we may write $\delta g/g = \delta\theta/\theta = \Delta N \cos i/b(\Delta\theta/2) = 0.16\,\Delta N$ and as ΔN may be read to within 10^{-4} cm., the sensitiveness is about 10^{-5}, the gram counterpoise in question presupposed. The error to be

apprehended from viscosity may be estimated from the following observations, figure 4, of the yield of the wire (ΔN, vertical) with the gram excess at the end of the balance beam. The graphs shown are for the 4th to the 7th twists, alternately in opposite directions. The others, eleven in all, were similar. The curves show that if measurement is made within 3 minutes after twisting, the error from viscosity, if ignored, would not exceed $\delta g/g = 3 \times 10^{-4}$. If, however, the coefficient of viscosity is known and allowed for, the error of 3×10^{-5} is improbable.

If the graphs be used to compute the absolute viscosity of the metal for the given twist and diameter, it is found to increase in 100 minutes from about 4×10^{19} to 3×10^{20} and thereafter is nearly constant. The twist was about 1.4° per linear centimeter; but in alternating, the effect of this is about doubled.

Observations on the Permanently Twisted Wire.—The readings were made two or three times daily, as a rule, for over a month. They were adequately recorded in graphs like figure 5, for example, figure 6 being the corresponding temperature graph. As the twist produced by the excess of weight of about 1 gram was here also about 25°, we have $\delta g/g = 1.6 \times 10^{-4}$ for each of the divisions (10^{-3} cm.) of the scale. The laboratory temperature was very variable from day to day; and the effect of this on the graph is at first sight disconcerting, though it contributes essentially to the interpretation. In figure 5 the initial mean rate of yield referable to viscosity, may be estimated as $\Delta N = 0.0009$ cm. per day. This makes $\delta g/g$ vary 0.00014 per day, a quantity in itself too large to be used as a correction, with confidence; after a month, however, this nearly vanishes, but a much more serious consideration, the thermal variation of rigidity, remains. It is this feature which makes the graph so exceedingly jagged. If we take the large drop between the 8th and the 10th day to estimate this effect, the data would be: $t = 22°, 15°, 20°$; $10^3 \Delta N = 8.1, 2.6, 8.7$ cm., which is equivalent to $\Delta N/\Delta t = 10^{-3}$ cm. per degree; or $\delta g/g = 2 \times 10^{-4}$, nearly, for each degree of fall of temperature promoting increased rigidity. Excellent observations on the torsional rigidity of iron and steel are given by Pisati (Landolt and Boernstein's tables). The coefficients are 2.1×10^{-4} for iron, and 2.3×10^{-4} for steel, therefore, even larger than the graph indicates. In fact, the present interferometer method could easily be modified to measure this constant accurately.

Summary.—In the above experiments as a whole, there is thus very little promise of arriving at the variations of g, with precision, by using torsional apparatus, so long as there is any marked change in the temperature of the environment. The discrepancy of the method is to be referred less to the viscosity of the metal and its thermal coefficient, than to the decrease of rigidity with rising temperature.

There is, however, one possible way out of the difficulty and this points

to the use of the alloys which Director C. E. Guillaume, of the Bureau International at Sévres, has spent so much time in perfecting. I shall hope to report observation with these in the future.

[1] Advance note from a Report to the Carnegie Institution of Washington, D. C.

THE GENUS BOTRYCHIUM AND ITS RELATIONSHIPS

By D. H. Campbell

Department of Botany, Leland Stanford Jr. University

Read before the Academy, April 27, 1920

The genus *Botrychium* comprises about thirty-five species of almost world wide distribution. Up to the present our knowledge of the life history of these ferns has been confined almost exclusively to two species, *B. lunaria* (L.) Sw., and *B. Virginianum* (L.) Sw.

In 1910, Dr. H. L. Lyon[1] published a brief account of the embryo of *B. obliquum* Mühl, which differed so much from the two species mentioned, that he proposed to separate *B. obliquum* as the type of a new genus, *Sceptridium*.

Dr. Lyon made quite complete collections of material of this species, and also prepared series of microtome sections of the gametophyte and young sporophyte. This material, as well as similar material of some other species, was collected in the vicinity of Minneapolis.

Dr. Lyon very generously turned over this valuable material to me, so that I have been able to make a fairly complete study of the early development of this interesting species.

The gametophyte of *B. obliquum*, like that of the other species, is subterranean. It closely resembles in its general structure that of *B. lunaria*, and *B. Virginianum*, and in size is intermediate between them.

The mature gametophyte is a somewhat flattened tuber, 3–6 mm. in length and about half as wide. Upon the upper side is a more or less conspicuous median ridge upon which the reproductive organs, archegonia and antheridia are borne. These closely resemble those of the other species, but the spermatozoids are considerably larger.

The embryo differs remarkably from that of the other species. A very conspicuous suspensor is developed, which is wanting in the other species, and the relations of the first leaf and root are different.

The fertilized egg becomes much elongated, and forces its way downward into the tissues of the gametophyte before any cell-division occurs. The first division is transverse, and cuts off a large basal cell, which becomes the suspensor. The smaller terminal cell gives rise to the organs of the embryo-sporophyte.

The further development of the embryo is more like that of the Marattiales, especially Danaea, than it is like the other species of Botrychium.

Danaea also develops a suspensor. The relative positions of the first leaf and primary root, also recall the embryo of some species of Ophioglossum.

As in Danaea and Ophioglossum, the young sporophyte of *B. obliquum* is bipolar in structure, the primary leaf or cotyledon and the primary root having a common axis traversed by a single vascular bundle which is continued from one into the other. The root is endogenous in origin, and pushes its way vertically downward, penetrating the foot which is practically destroyed. The stem apex is very inconspicuous, and is situated between the cotyledon and the primary root. No stelar tissue is developed from the stem-apex, and the vascular system of the young sporophyte is made up exclusively of a union of vascular strands belonging to the leaves and roots.

The primary leaf is ternate in outline, and closely resembles the first foliage leaf of Helminthostachys.

The embryo of *B. obliquum* differs from that of *B. lunaria* in several important particulars. The embryo of the latter is in its early stages composed almost entirely of the foot and root between which is the very inconspicuous stem-apex. The early leaves are reduced to mere scales, and it is several years before the first green leaf is formed.

B. Virginianum, on the other hand, has the primary leaf even better developed than it is in *B. obliquum*, but the root is of external origin, the very large foot is persistent, and the cotyledon makes a sharp angle with the root.

B. obliquum agrees with Helminthostachys in the development of a suspensor, but the position of the organs of the embryo in the latter is more like *B. Virginianum*. In Helminthostachys, also, the first leaf is rudimentary. The second leaf is functional and closely resembles that of *B. obliquum*.

It looks as if *B. obliquum* were more generalized than the other species, suggesting a relationship on one hand with Ophioglossum, on the other approaching Helminthostachys, as well as the Marattiaceae, especially Danaea.

The differences in the embryos of the three investigated species of Botrychium, are accompanied by significant differences in the adult sporophytes, probably sufficient, together, to warrant a separation of the genus into three genera.

The "Ternatum" division of the genus Botrychium to which *B. obliquum* belongs, would probably constitute the genus Sceptridium, Lyon; Blunaria, the type species, and its allies, would retain the generic name Botrychium Sw., while *B. Virginianum* would be the type of Osmundopteris, Milde.

[1] A New Genus of Ophiolglossaceae, *Bot. Gaz., Chicago*, 40, 1905 (455).